DAMAGE BOOK

THE SCIENTIST IN ACTION

The
SCIENTIST IN ACTION
A SCIENTIFIC STUDY OF HIS METHODS

by

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TO THE MEMORY OF THE LATE

PROF. EDWIN H. BARTON, F.R.S.,

IN WHOSE LABORATORY I FIRST RESEARCHED.

"If in some things I dissent from others, whose wit, industry, diligence, and judgement, I look up at and admire, let me not therefore hear presently of ingratitude and rashness. For I thank those that have taught me, and ever will; but yet dare not think the scope of their labour and inquiry was to envy their posterity what they also could add and find out. . . . If I err, pardon me: 'No art is discovered at once and absolutely.' I do not desire to be equal to those that went before; but to have my reason examined with theirs. . . . If I have anything right, defend it as Truth's, not mine, save as it conduceth to a common good. . . . Stand for truth, and 'tis enough."—BEN JONSON, *Discoveries Made on Men and Matter* (1641).

PREFACE

When scientists are at work some of the things they do differ according to the branch of science involved, but others show common similarities independent of the particular science. In the present book an attempt has been made to describe these common similarities in plans of action and results of scientific research. The use of the scientific devices described is the same in academic or industrial, in biological or non-biological research. The scientists' methods of research have recently become of the greatest importance for two quite different reasons.

Among scientists the study of scientific method has been unpopular. The research worker has been apt to say, 'Never mind about the methods. It is the results that matter.' But now, in that most advanced science, mathematical physics, research papers have been published in which an 'observer' appears as an essential part of the 'result.' The scientist observer is actually being regarded also as a disturbing influence upon the phenomena studied. To talk about science without talking also about the scientist is rapidly becoming meaningless. This aspect of research technique is of special interest to scientists.

There is, however, a much wider aspect which affects both scientist and non-scientist. The results of scientific research are being applied in practical affairs so as profoundly to alter civilization and to affect human happiness. It has been suggested that mankind might enjoy the blessings of science with less of its curses if not only scientific facts were used, but if also the kind of action used by scientists in getting those facts were directed to social uses. To the reader interested in social problems I offer the book as an account of this special kind of human (some may call it inhuman—Chapter IV) action. Throughout, I have tried to express the relationships between scientific and ordinary action. It seems that the only scientific device which is not widely applicable is the special experimental technique necessary to establish the cause-and-effect relationship.

The point of view taken up in this book is throughout uncompromisingly non-philosophical. Nothing could be more unrepresentative of the viewpoint of the vast majority of research workers than an analysis of scientific research in terms of philosophy. But although science without philosophy is the rule followed in the research papers published in the 36,000 current scientific journals of the world, in general science writing it is the exception. If an endeavour is made to separate science from philosophy by consulting the literature of scientific method, philosophy will be found there also. I have, however, abandoned the traditional view and treated scientific research as a problem in human action.

If incidentally the complete logical independence of science and philosophy has been shown, I do not suggest that either philosophy or science is illogical. They are different ways of looking at things. To claim that one is more fundamental than the other is logically unsound. To claim that one is better than the other is to apply assessment of value, and is therefore unscientific. Wherever the treatment seems unconventional the reader is asked to remember that I am concerned always with what research workers can be observed to do, and am non-committal on the relationship between thought and action. Observable achievements rather than unobservable inferences, aims, ideals, or beliefs are taken as basic.

Elsewhere wide publicity has been given to the 'Absolute Truth' theory of science according to which research technique is regarded as a means of getting to 'the bottom of things' or of approaching that God-like knowledge called Absolute Truth. For all I know science in general and my book in particular may be full of absolute truths, but as Huxley put it "Science commits suicide when it adopts a creed." As a scientist I cannot use the philosophical ideas of 'truth' and 'validity,' I therefore challenge devotees of the Absolute to produce not their evidence, but a description of the tests, based upon agreement between human observers, by which the Absolute can be recognized when it is met.

In its passionate and challenging devotion to facts science

seems to be distinguished from all other human activities. Science neither has nor desires any protection whatever against statements of fact. A passionate devotion to statement of fact outside of science may alienate a man from his friends if not put him in a law court, prison, concentration camp, or before a firing squad. In certain circumstances statement of fact without comment is libel or slander. Religion is protected by laws of blasphemy. Even militarism appears to be a delicate flower needing special legal protection against statement of fact even without comment. In science it is never impolite, immoral, disloyal, unpatriotic, or 'not done' to state facts. To criticize the official statements of a state president may according to the person, place, and period be a way of committing suicide. The official statements of a President of the Royal Society challenge critical examination by all.

Any analysis of the scientist in action will depend in part upon the analyser, and one logically sound way of looking at the subject does not logically exclude another and different way. After all to the external observer a book starts as an elaborate series of 'overt responses' evoked from an animal pushing a pen over many sheets of paper. Without a reader it remains a lot of irregular black marks on a regular pile of papers. If it means more than that to a reader, then that something more is his contribution. I ask the reader to approach this treatment of science-without-philosophy in the spirit of Faraday's words, "We may be sure of facts, but our interpretation of facts we should doubt. He is the wisest" who can take "a fact for a fact, and a supposition for a supposition, as much as possible keeping his mind free from all source of prejudice."

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THE SCIENTIFIC OUTLOOK

"It was a great step in science when men became convinced that, in order to understand the nature of things, they must begin by asking, not whether a thing is good or bad, noxious or beneficial, but of what kind it is? and how much is there of it? Quality and Quantity were then first recognized as the primary features to be observed in scientific inquiry."

CLERK MAXWELL, *British Association Address* 1870.

CHAPTER I

FOUR QUALITIES OF SCIENTIFIC RESEARCH

THE first requirement in a piece of scientific research is a research worker. This elaborate and complex piece of apparatus is essential whether the research be biological or non-biological, academic or industrial, important or unimportant. When the research worker has been found he has to take action, for thinking about things is not by itself the whole of research technique. In referring to human action, the word will be used to describe anything a man may do, which can, with the current technique, be observed by others using coincidence observation (Chapter VI). It follows by definition that speaking, writing, or manipulation of apparatus in a laboratory are forms of action but thinking, believing, or feeling are not.

It will be taken as basic, that scientific research is a form of human action. One consequence of taking this point of view is that in interpreting the results of research it becomes necessary to examine to what extent they depend upon the human factor. It becomes necessary to examine not only the products but also the producers of scientific research. No very detailed knowledge of research workers is necessary to discover that they remain human always. They are not a special kind of human being having the qualities of Huxley's ideal intellect, which he described as a clear, cold, logic engine. No scientist is scientific always. Detailed examination of research technique shows also that apart from the few exceptions discussed in Chapters IV and XV actions in research show some similarity with ordinary human actions.

Two restrictions are observed throughout the book. Firstly, the discussions are confined to devices of research technique applicable in all the sciences. Laboratory, observatory, or field technique used only in particular sciences are not therefore discussed. Secondly, no philosophical treatment is given to any of the subject matter. When such

questions as, Is an electron real? are discussed, it will be seen that the interest is in human action and reaction towards the subject matter.

The four qualities of scientific research first selected for general discussion are:

(1) *Action.*—Scientific research is a form of human action which gives two typical products.
(2) *Facts.*—The first product is facts (judgments of coincidence or coincidence observations, Chapter VI).
(3) *The Arrangement of Facts.*—The second kind of product is such arrangements or patterns of facts as scientific classifications, laws, and theories.
(4) *Newness.*—An essential quality of all research work is newness in the sense defined later in this chapter.

The achievement of scientific research is twofold: the establishment or discovery of facts; and the arrangement of these facts in various ways (as, for example, facts about the physical properties of gases are arranged in terms of a kinetic theory of gases). This twofold achievement can be analysed conveniently by using two ideas, (i) observed *coincidences* and (ii) *patterns* or wholes into which observed coincidences can be fitted.

FACTS AS THE BASIS OF SCIENCE

It is customary to discuss the results of scientific research in terms of the idea of a unique and final kind of knowledge called truth or absolute truth. The basic idea of this viewpoint seems to be that a scientist is a kind of collecting agent. His human properties only affect scientific knowledge in such factors as affect his use of speech and writing. The idea that a scientist is a vital part of scientific knowledge is quite foreign to this truth viewpoint. In this book, however, the scientist is made the creator of science. That part of science which does not change with time is called facts, and is made the basis of the whole discussion. This and the truth viewpoint will be compared in Chapter II.

But what is a fact? As often happens with a familiar idea,

there is considerable difficulty in defining it. There is, however, no doubt whatever about one of the properties of a scientific fact. It is a piece of impersonal knowledge. If facts are impersonal knowledge are they therefore something independent of human beings? Must such a statement of fact as 'The density of water at 20° C. is 0·998 gm/c.c.' be regarded as something absolute, something which would be true even if the whole of mankind were to vanish? The view taken here is that such questions are at present outside the scope of scientific research. There is not a single fact, law, classification, theory, or relationship of cause and effect which has not been a biological product. To remove the human element is to remove science. Moreover, the scientist's task is endless. When Newton formulated his law of universal gravitation he did not reduce by one the number of absolute truths to be discovered. He created a new pattern into which facts could be fitted. Einstein created still another pattern into which these same facts, together with others, could be fitted. Some of the additional facts such as those connected with the motion of the planet Mercury or the appearance of two stars seen first on the same side and then on opposite sides of the sun could not be fitted into Newton's pattern. In the future will not Zweistein devise another pattern, followed later by Dreistein's and so *ad infinitum*? If you ask, is not Einstein's a better theory than Newton's, is it not nearer to the truth? I would reply, if the time available for discussion were short, Einstein's theory accounts for more facts than Newton's, or Einstein's theory is a pattern which contains more observed coincidences than Newton's. Better is a word of too vague or imprecise meaning to be used in scientific language. If the time available for discussion were longer I would ask you to explain in detail what was your analysis of the qualities which constitute a good scientific theory. These are discussed later. If the question was as to the nearness to truth of the two theories I could only ask what are the various means by which nearness to truth is to be tested. These questions, which are discussed later, would be asked not as rhetorical devices but as serious

inquiries. There would be no suggestion that they could be answered briefly. For the present they must be left while the discussion of fact is continued.

Facts are not independent of human judgments, but they are impersonal in the sense that they are independent of the judgments of any *one* man. Scientific facts are not essentially dependent for their use in the scientific world upon the work of any individual man. Although scientific facts and scientific patterns of facts are discovered and formulated by individual men, they have to be presented in such detail that other men with suitable laboratory and mental equipment may examine the alleged facts for themselves. The name of the individual discoverer is often attached to the name of a scientific fact. One of the phenomena of light is called Newton's Rings, but this is merely for convenience of reference. Any normal man, having suitably arranged apparatus, can see the phenomenon for himself and every degree student studying Physics in a university actually does arrange suitable apparatus and sees the rings for himself. Not only in facts but in the patterns into which facts are arranged is this same impersonality found. Once Newton had arranged a huge number of facts into the simple pattern called the Newtonian Law of Gravitation and had published the result to the world at large, then the pattern lost its Isaac-Newtonallity. It became an impersonal piece of knowledge which can be understood, tested and applied by many men. To use a fact or theory in research work it is quite unnecessary to know who discovered the fact or who devised the theory. This is in striking contrast with the wise use of an opinion, where knowledge of the originator is often highly desirable. An opinion is essentially attached to some individual or group of individuals. A scientific fact is essentially not attached to any particular individual or group of individuals.

The technique of scientific research is the only device known to man at the present time by which a fact may be established. Genius may produce a great idea but no amount of genius will alone produce a fact. The technique of scientific research has to be applied to a great idea before it

becomes a great scientific fact or theory. Since the aim in this book is to build upon observed coincidences, things which more than one man may observe, caution must be used in speaking of ideas. On the present view an idea yields something to the thinker only and cannot be observed by other men. When the idea is combined with action it may or may not lead to one of many things, any of which can be observed by other men. A great idea combined with action may yield a work of art, a symphony, a picture, a statue, a poem, or a novel. It may give a road, a bridge, a machine, a building, or an increase in yield of a crop of foodstuffs. It may produce a religion, or a scientific theory, or a statement of fact. The link between the idea and the statement of fact is part of what is here called the technique of scientific research. Before all else, then, the technique of scientific research has this one unique property. It is the only tool at present available by which a fact may be established. Although the precise definition of a fact will be deferred until observation is dealt with in Chapter VI, future discussion may be anticipated by stating that the word is used to mean an observed coincidence. This is any kind of sense datum got by using elementary human judgments in which agreement between different independent observers can be reached without the use of threats or torture, mental or physical. The substitution of this idea for the philosophical idea of truth must be regarded as an arbitrary choice made to use as basic, a kind of knowledge known to be accessible to man at the present time. The choice is neither more nor less arbitrary than the usual one, using the idea of truth or absolute truth as basic. The alternative is not intended as a criticism or as an assessment of the value of philosophical or other viewpoints.

THE ARRANGEMENT OF FACTS

So far two points have been emphasized. That scientific research is a form of human action and that this particular kind of action gives facts, which are closely associated with observations of coincidences like that of a pointer on a scale-

mark, or the coincidences of the hands of a clock and the markings of the clock face. A third feature of scientific research is that it often gives patterns of the facts. By this is meant that the scientist attempts to group facts together into wholes. The facts are not regarded as so many isolated things. If possible they are seen, or thought of as being connected in some way, as for example by classifications, laws, or theories or as cause and effect. A scientific theory is a kind of framework into which a large number of facts may be fitted. Before the theory is stated a number of facts are known; it is noticed that certain things always happen in certain circumstances, but we have to think of them and remember them as so many isolated happenings. As soon as the theory is stated all these separate facts seem to fit into a unified whole. The individual facts can almost be forgotten. The theory seems to be a sort of pattern into which the facts fit. If one of them is wanted it can readily be found in the pattern. But a scientific theory seems to mean more to us than a convenient filing system for a number of facts. An alphabetical card index of addresses is a convenient way of arranging a number of separate things so that any one of them can readily be found.

But a scientific theory seems to mean more than the sum total of the separate facts which it covers. The arrangement of facts in the framework or theory often starts the search for new facts. It is as if gaps are seen, to fill which we have no facts. The place of the gap in the whole theory suggests where in Nature the facts may be found. The perception of the gaps gives a new point of view followed by new action. Such an arrangement of facts as a theory is both a product and a tool of research. There is much to be said in favour of regarding a theory as a policy of action rather than a creed. It will be seen from the chapters on scientific theory that although a scientific theory is regarded as a thing of beauty, enjoyed and valued quite apart from its utility as a filing system and its utility as a predicting oracle, reverence is reserved for facts rather than for theories. Here is Faraday's view expressed on two different occasions. The first in a letter written from the Royal Institution on October 21,

1856: "For my part, I think that as facts are the foundation of science, however they may be interpreted, so they are most valuable, and often more so than the interpretations founded upon them." The second occasion was in a lecture. "We may be *sure* of facts, but our interpretation of facts we should doubt. He is the wisest philosopher who holds his theory with some doubt; who is able to proportion his judgment and confidence to the value of the evidence set before him, taking a fact for a fact, and a supposition for a supposition; as much as possible keeping his mind free from all source of prejudice, or where he cannot do this (as in the case of a theory) remembering that such a source is there."*

NEWNESS

To the three properties, action, fact, and pattern, a fourth may be added. This can best be found by asking, What do you consider to be the most striking common features of great scientific discoveries? What properties in common have the greatest discoveries of men of scientific genius? Think of the names and work of the Presidents of the Royal Society and of Nobel Laureates in any particular Science. First and foremost their work is based on fact. If it is a theory in mathematical physics, no matter how beautiful a piece of mathematics it may be, at some point it makes contact with and fits some experimental observations. It agrees with some observed coincidences. But this has already been included as one of the properties of scientific research. The work of these men of genius is based on fact, on observed coincidences. What other common feature is found in their work which covers as a whole so diverse matters? I submit that this feature next in importance to fact is *newness*. At the time of its publication their work is new. If it is a fact it is a hitherto unsuspected or unestablished fact. If it is a theory it is an essentially new theory, an essentially different pattern into which older facts can

* Gladstone's *Life of Faraday*, p. 95. London, 1874. Faraday's use of the term *philosopher* corresponds with the present-day use of *scientist*.

be fitted. Often the new fact is one which will not fit into a current theory or pattern of facts.

To illustrate more clearly what kind of newness is meant here let us suppose that the study of a certain physical phenomenon led to the theory that it could best be described in terms of an attractive force proportional to the power 3/2 of a distance. Next suppose that the subject was re-examined, perhaps more facts were available about the phenomenon, and that it was found that the total facts could best be explained in terms of an attractive force proportional to the power 4/3 of the same distance. Although these two theories differ I should not call the second one essentially new. It is not a new way of looking at the phenomenon but is rather an extension of the old theory. The formulator of essentially new theories benefits by the knowledge accumulated by his predecessors but his theory is not built upon old theories. Although this statement is slightly different from one often expressed it seems to fit the facts better. On this view Einstein's General Relativity Theory is not a theory which has grown out of, or developed from, or evolved from Newton's work, it is a different way of looking at things. This is what I mean by essentially new. On this view the majority of published research is not essentially new. Every piece of research has, however, something new in it. Newness is a property of all research which can be seen most readily by examining that research of men of genius which is called their greatest or best work. Two actual examples will be given, one from atomic physics and one from bio-chemistry.

At one time many of the phenomena of so-called atomic physics could be described or explained in terms of the electron, which was given the properties of a particle of electricity, size, mass, and electric charge, quantities which could be defined from the classical experiments of Sir J. J. Thomson in 1895. The atomic nature of these particles was postulated to account for the fact that the smallest electrical charges observed were either one or more integral multiples of an experimentally determined quantity. The postulated qualities served admirably to account for many phenomena

observed when electricity was passed through rarefied gases, when metals were irradiated in a vacuum with light or X-rays, or when they were heated; they also served to explain many phenomena shown by radio-active substances. Later an elaborate theory of the optical and electrical properties of matter, including a model of the atom, was built upon these same postulated qualities of the electron regarded as a particle of electricity. The agreement between experiment and theory was very good indeed over a wide range of phenomena, but not in dealing with some phenomena. To take account of these and of some optical phenomena de Broglie postulated quite a new property of the electron in associating with it some type of wave. The experiments of Davison and Germer, in which it was found that when a stream of electrons was directed suitably on to a nickel crystal in a vacuum, then regular reflection of the electrons could be observed, was a type of experiment for which the purely particle concept of the electron was inadequate. The work both of de Broglie and of Davison and Germer was essentially new in the sense in which the term is here used. Their ideas or experimentally observed facts did not at the time fit into current ideas. Although every piece of research work establishes new knowledge, the term essentially new will be used only for work of this type which does not at first fit into established knowledge.

The second example of what is meant here by essentially new in a piece of research is taken from bio-chemistry and relates to the discovery of accessory food factors which are now called vitamins. Probably as far back as records go men have always regarded illness as due to the presence of some additional and harmful factor in the man who was ill. At first it was a devil or devils who had to be cast out before the man could be restored to health. Later it became humors, and until recent times the presence of some additional factor not present in health was regarded as essential for the presence of disease. Drugs, medicine, and treatment generally were regarded as neutralizing or counteracting the effects of the harmful things whose presence were pro-

ducing the illness. A disease such as scurvy,* often observed in seafaring folk who for long periods were deprived of fresh foodstuffs, was regarded even recently as due to bacteria or poisons from putrefying meat or fish; that is, the disease was thought to be due to the presence of some factor absent in health. A similar view was taken of beri-beri. Eijkman, a military prison doctor in the Dutch Indies, noted that many of the natives whose diet consisted largely of polished rice developed the disease of beri-beri. He noted also that fowls, pigeons, and ducks fed on polished rice developed symptoms similar to those of the natives. During his period of office a new chef was appointed and decided that the prison hens belonged to the civil population; not to the military population. Their ration of polished rice was accordingly reduced and the ration of barley increased, and Eijkman noted with this change a disappearance of the beri-beri symptoms. He also noted that the addition of the rice polishings to the diet of polished rice was also followed by a disappearance of the symptoms even without the use of barley in the diet. He concluded, again in accordance with the contemporary view of disease in general as due to the presence of some factor, that foods such as rice, over-rich in starch, produced in the intestine a substance which acted as a poison to nerve cells, for which the outer layers of the rice grain contain an antidote. Later another Dutch worker, Grijns, in 1901, interpreted these results differently by suggesting that beri-beri, whether in birds or man, was due to a 'deficiency'—to the *absence* from the food of an essential factor. This is the present-day view owing largely to the classical experiments and interpretation of Hopkins in 1912. These biological experiments, carried out as a whole with a beauty of experimental technique similar to that of Faraday in non-biological experiments, have laid the foundation of the current view of certain diseases, including scurvy, in regarding them as due to the *absence* of certain factors, instead of the traditional view that all disease was due to the presence of certain factors. The

* *Vitamins: A Survey of Present Knowledge*. Medical Research Council Report. London, 1932.

establishment of this view, which did not fit into the older view, is an illustration of the sense in which 'essentially new' is used when speaking of a piece of research. Although the minuteness of the amounts of vitamins necessary to maintain health has the quality of astonishing, this knowledge is not essentially new because very minute quantities only of certain chemical elements or inorganic chemical compounds were already known to be sufficient for health. If vitamins were shown to behave in the body as catalysts then this would not be 'essentially new' because many examples of catalysis are already known. To show that vitamin x can be divided into vitamins $x_1, x_2, \ldots x_n$ is not essentially new in the sense in which we are using the term. All new knowledge is not regarded as essentially new. Only newly established results which do not fit into current beliefs are so regarded. No clearly defined distinction can be drawn between new and essentially new in all researches but these two examples, the wave properties of the electron and the idea that absence of a factor rather than its presence could be followed by illness, both illustrate well the newness characteristic of all research.

Reaction towards the New in Everyday Life.—Of the four qualities of research work selected for the introductory discussion, the first relating to action and the fourth relating to newness are less generally discussed both in this book and elsewhere than are the other two. By regarding research as a form of action, and by noting that research is necessarily concerned with new things it becomes relevant to ask how we act towards new things in everyday life. This question relating as it does to anything new involves so many different topics that it is impossible to present any serious study of it briefly. Instead I offer a generalization which appears to fit the facts. It may be called the Attack-Escape Principle and may be expressed: On most occasions when most adults are first conscious of something new they either attack or try to escape from it. New means new to the individual concerned and refers to unfamiliarity rather than to any time factor. A new object can cease to be new

to an individual in a few moments. Attack includes such mild action as laughter or any form of ridicule. Escape includes putting out of mind, repeatedly forgetting, or, as some would say, banishing to the unconscious.

The principle seems to apply to all manner of things material and non-material. Under conditions of civilization the attack is often accompanied by rationalizations of the action, that is to say, the individual gives his 'reasons' for the attack even if he is first violent enough to destroy the new machine, for example. I neither state nor imply that the attack or escape is *because* the thing is new. The generalization, apart from the reference to forgetting, deals strictly with what can be observed, not why the particular reactions take place. In a detailed presentation I should not talk about an individual repeatedly forgetting but should refer only to such externally observable actions as starting, blushing, increased pulse or breathing rate, or secretion of adrenaline or changes in metabolism which, with the aid of suitable experimental technique, could be observed each time the new idea or object was presented to the individual. The principle refers only to adult action but apparently fits savage as well as civilized men and women. The experiments of the behaviourist psychologists on infants whose reflexes or responses have not been 'conditioned' show that rather the opposite of the attack-escape principle fits infant behaviour.

The reception of Planck's quantum idea in 1900, when it was first published and was new, is an example of the operation of the attack-escape mechanism in research. If the mechanism fits the facts of human action, its relevance in discussing the extreme rareness of such new ideas as were used in quantum or vitamin research is readily apparent. The reader who is interested in deciding whether the hypothesis fits the facts is invited to examine the reception in all periods of human history of essentially new kinds of food, housing, hygiene, clothing, sex behaviour, means of transport, treatment of disease as well as essentially new social or political policies, new religions, new painting, sculpture, poetry, drama, fiction, music, or the expression

of any kind of new idea. The rationalization of the attack on the first man carrying an umbrella in London differed from the rationalization of the 1900 attack on Planck's new quantum idea but the attack was there all the same.

A NOTE.—Scientific research is not itself a science; it is still an art or craft and "No art is discovered at once and absolutely." Any analysis of the characteristic behaviour of scientists in action will depend partly on the analyser, and one logically sound way of looking at the subject does not logically exclude another and different way. The whole subject is highly controversial. If any of the statements on these controversial matters seem dogmatic, this is quite unintentional and is an element present only by accident in an effort to avoid vagueness of exposition. There is a very useful place in research and thought for vague thinking. New ideas do not suddenly appear with no initial vague brooding over the subject. Although a new idea in definite form seems suddenly to flash through the mind, the thinker seems to go through an earlier stage of vague thinking on the subject, when he could not express to others what he is thinking. Vagueness has its place in research technique and thought but not in written or spoken expositions intended to be considered seriously by more than the author. Words, as distinct from mathematical symbols, are sometimes of so elusive a meaning as to interfere with the application of pure reason to problems. If the reader meets with a statement which he regards as dogmatic he is asked to apply pure reason to it, forgiving the author his quite unintentional dogmatism.

CHAPTER II

DESCRIPTION *VERSUS* ABSOLUTE TRUTH

THE description of research as a form of action, rather than as a way of thinking about things, is not common and differs from that given in the standard treatments of scientific method. To discuss the relationship of thought to action and the relationship of absolute truth to description may serve to give an idea of the general point of view to be used throughout the book. This discussion may serve to show that the differences of treatment between the present and the standard works are due to my adopting a fundamentally different attitude and not to differences in detail or application of an attitude shared in common.

This treatment of research starts from observation or in general from things of which the research worker is conscious. An arbitrary division into two classes is made of all the elements of consciousness. One category, containing all the basic data of science, concerns the sense data got by judgments of coincidence (coincidence observations). The other contains all that is not observable by this judgment of coincidence. The arbitrary division is made *solely* to separate those things about which agreement between different observers is in practice reached, from those things which from their very nature can be observed only by the individual himself, and about which agreement between different observers is not in practice reached. The second division contains such varied experiences as, for example, thinking, believing, wanting, willing, and consciousness of emotions.

When research is described as a form of human action, is not that equivalent to saying that research is a way of thinking about things? If action were determined solely by thought the two statements might be equivalent. The language of everyday life and the classical treatments of scientific method all seem to use the idea that man is a reasoning creature in the sense that consciousness of a

'reason' for doing some action is the determining factor. It is regarded as possible for any normal man to give 'reasons' for many of his actions and these statements are regarded as expressing the factors *determining* his observable actions.

There are, however, a number of facts which do not fit into this 'Reasons' theory of human action. For example it is sometimes found that the reasons given by the same man for doing the same action differ according to the occasions and also according to the person to whom he gives the reasons. Moreover, he sometimes makes 'slips' of the tongue or of the pen in giving his reasons. There are also actions which are first accompanied by some kind of consciousness and later occur without the consciousness when they have been 'learnt' or when a habit has been formed. It seems more in accord with observed facts to use the idea that consciousness may or may not accompany action and that 'reasons' are things or statements of things which may or may not precede, accompany, or follow actions. Reasons are not like those of causes of actions which are determinable by the presence and absence type of experimental technique. The facts seem to be fitted better by the idea that reasons are *rationalizations*. They frequently contain concepts devised to be attached to such observable actions as do not fit into the Rationality or Pure Reason theory of human action in order to make some kind of link between the square actions and the round gaps in the theory.

In dividing the things of which a research worker may be conscious into two classes I may be asked if these two are to be kept apart throughout the rest of the book. In reply it may be said that the attempt is made to regard them as separate but parallel. Research is to be regarded as a twofold process which yields facts and arrangements of facts. When some arrangement is found into which many facts fit, any further statement about the relation of the facts, and the arrangement or pattern, is regarded as philosophical or emotional and therefore outside the scope of present-day research technique. Research work is to be regarded as a double process of using sense organs to observe

and to get coincidence observations, and of using imagination and pure reason to build up ideas or mental concepts into which the observations can be fitted. Sometimes facts (coincidence observations) are got which cannot be fitted into any known conceptual scheme. Sometimes a conceptual scheme is got which will not fit the facts but research workers like, and try, to get these two different activities moving together along parallel tracks. They do not like accumulations of facts fitting into no known mental patterns or models and they do not like mental concepts into which no facts fit. When the desired state is reached so that both facts and scheme have been obtained, research technique as I understand it takes us no further. Usually the technical accounts of research published in the scientific journals of the world contain no other than these two types of result. In so far as may be judged, the majority of research workers show no interest in any other statements about the results of research work.

NON-TECHNICAL INTERPRETATIONS OF SCIENCE

Unfortunately most published research papers in any one science are so written (chiefly for brevity's sake) that research workers in other sciences cannot easily separate the facts from what are usually called inferences. Furthermore almost the whole of research work published in scientific journals is almost if not quite incomprehensible to the reader who has had no scientific training. Now many remarkable changes produced in the way civilized men live nowadays originate in the subject matter of these incomprehensible research papers. If the layman wants to know anything about their contents he is compelled to get the information from popular science writing based upon the research papers. The state of things is rather like that in the days when men were obliged from lack of bibles or literacy to get their religion from interpreters or priests. In popular science writing the majority of scientists take up one of two attitudes towards the research results. They refer to the results in one of two ways.

For convenience of reference these two attitudes will be called the Description or Patterning, and the Absolute or Reality theories of scientific research. The first view is in essence that indicated earlier in this section and is the view adopted throughout this book. Users of this theory or point of view will have nothing to do with the unlabelled mixing of philosophical and scientific statements. Users of the Absolute theory on the other hand speak of the results of successful scientific research as if they represented what actual objects and events really and truly looked like or how they behaved. They speak of science as if it were a means of 'getting to the bottom of things' or as if it gave a kind of superhuman or God-like knowledge.

The majority of research workers do not write non-technical accounts of research work, but the minority includes men of the highest scientific rank, men of scientific genius, as well as ordinary research workers. It so happens that in the majority of these works the writers have all adopted the Absolute view of scientific research. The result is not so much a non-technical account of what they themselves or other research workers have done, as an intimate mixture of this with all kinds of philosophical and emotional interpretations. Even some research workers who have read both the original research papers and the popular writing upon their content, have the greatest difficulty in separating the science from the mixture. In one example a purely mathematical concept used to deal with certain experimental results in physics appears in the non-technical account as a rationalization of belief in the necessity of including the idea of free-will in any theory of human action which would fit the facts. This Principle of Indeterminacy is used in a purely mathematical theory having at present no scientific connection whatever with the study of human action.

If this use of either the Description or the Absolute views of scientific research affects only the non-technical writing, why pay any attention to it in a book devoted to the serious study of research technique? The reply is intended solely for the reader interested in the possible application of

research technique to certain problems of civilization. Now in some of this non-technical scientific writing it has been said by one of the leading scientists of the day that research technique applied to certain problems in physics leads to the result "Something unknown is doing we don't know what." If the application of research technique in the laboratory gives that kind of result, then it is unreasonable to expect it to help outside the laboratory in solving problems of civilization. But no account of research in physics or any other subject, published in a scientific journal, has ever given such a result. The statement is a terminological inexactitude appearing only in non-technical literature. Yet the way in which non-technical scientific writing is carried out is of vital importance to the layman who has no other source of information as to the results obtained by research workers. The authority of the writers of this type of untrue statement is so great that counter-authority is needed to convince the layman that all scientists do not agree with them. Here therefore is what Einstein says: "There are scientific writers in England who are illogical and romantic in their popular books, but in their scientific work they are acute logical reasoners." He warns the reader "You must distinguish between the physicist and the *litterateur* when both professions are combined in one."* There is, however, very great difficulty in separating the scientific from the philosophical and romantic and the reading of the original research papers written in highly technical language is often a necessary part of the task.

The main difference in point of view between writers who use the Absolute theory and those who use the Description theory of scientific research is seen in the way they write about the relationships of scientific theories or other scientific arrangements of facts to other aspects of human life and thought. Until this part of the subject is reached there is I think no difference in the two viewpoints. Neither type of writer is interested in a particular theory unless it is logically sound and fits those facts for which it was devised.

* Part of a discussion given in full in Max Planck's *Where is Science Going?* p. 211. London, 1933.

The crux lies in the implied claim that the technical verification of a theory also gives logical authority for making philosophical statements about the theory as a whole or about the postulates of the theory. For example, if the idea of an electron with certain postulated qualities is used in a theory and the theory is found to fit the facts, then this, on the Absolute view, is taken as logical justification for using, in talking about an electron, such purely philosophical terms as 'real' and 'existence.'

In examining original research papers one has to compare their mathematical equations with the things postulated or imagined by the theorizer. These statements usually begin 'Let it be supposed that . . .' or some such form. In all the examples which I have examined these statements never say anything about 'real' or 'unreal.' If one added to the postulates an additional one in the form 'Let it be supposed that the electron is real' (or unreal according to philosophical taste) then it will be found that no additional equation can be written and not the slightest change has to be made in the mathematical treatment already given without this additional postulate. If this purely philosophical idea of 'real' or 'unreal' is ignored in doing the piece of research it is difficult to see how it can logically, within the scope of science, be attached to the products of the research when all the work is done. Nevertheless this type of claim appears to be made in all seriousness by the supporters of the Absolute theory of research and it is foisted onto the non-scientific reader in many popular books. It has also been claimed that if such a mathematical form as a differential equation represents certain metrical aspects of a phenomenon, it therefore represents the Inner Reality of the phenomenon.

SCIENCE AS DESCRIPTION AND PATTERNING

On the Description or Patterning theory of research used in this book a technically verified theory is regarded as one way of looking at the facts covered by the theory. The theory is a kind of map which helps the research worker

in his action. It is a policy of action, not a creed of thought. No claim is made that a technically verified theory is the only way of looking at the facts or is even the only scientific way of looking at them. A technically verified theory is a way of patterning the facts, and different research workers may find different ways helpful to them. To some workers postulates which can be visualized may be necessary. Other workers may be able to use mathematical patterning devices which cannot be visualized. Metrical aspects of some object or phenomenon may be dealt with by one kind of theory, and non-metrical aspects of the same object or phenomenon by another kind of theory. No claim is made that metrical knowledge is the one and only kind of scientific knowledge.

The user of the Description or Patterning theory is under no logical obligation whatever to justify his use, according to convenience, of two or more theories relating to facts about the same objects or events. He makes no claim that a technically verified theory represents Absolute Truth or God-like knowledge. If he finds that mathematics helps him most to pattern the kind of facts in which he is most interested he does not proceed to elect the Almighty the most senior of all Senior Wranglers *honoris causa*. If he sees order in some selected facts he formulates a natural law but does not claim that the law was also formulated by the Creator, neither does he say that concrete objects or events 'obey' the law. If he sees similarities in certain selected facts he makes a classification but does not call some classifications artificial and some natural. All classifications are regarded as artifices. That kind of patterning of facts called a causal relationship, is found solely by the use of the technique of experiment and a cause is not regarded as something which gets inside and makes a phenomenon happen.

As I see the user of the Patterning theory, he is a Doubting Thomas. To adopt Robert Boyle's term of 1661, he is Sceptical Scientist. He adopts Faraday's view expressed in a lecture: "He is wisest . . . who holds his theory with some doubt . . . taking a fact for a fact, and a supposition for a supposition; as much as possible keeping his mind free

om all source of prejudice, or, where he cannot do this ιs in the case of a theory), remembering that such a source there."* The nearest he gets to certainty is in facts (coindence observations) and he does not talk about having .ith or believing in these observations. Instead he says that e finds that they remain constant. He has no deep inner onvictions not even about his coincidence observations nd has no qualms about suspecting his facts and reobserving them. His greatest delight is in the patterning of .cts. He is thrilled by the fit of facts, but when irritated y the lack of fit of facts into the patterning devices used ι research he seeks either more facts or a different patterning evice.

No matter how much he loves his facts he soon ceases) love them singly. Technically verified laws, causal relaonships and theories have for him a beauty and fascinaon all their own. It never occurs to him to pretend that is ways of looking at facts are also the ways used by the 'reator. It is in these ways that he differs most from users f the Absolute theory. As he does not pretend that any f his particular arrangements of facts have the unchanging nality of Absolute Truth he is less reluctant to try many ifferent arrangements. He agrees in insisting that a scienfic theory must be logically sound and must fit the facts, ut he does not mind how fantastic its postulates may be.)nce the two fundamental requirements are fulfilled he sks only such questions as 'Does the theory help as a policy f action?' or 'Do the facts arranged in that particular way ive me aesthetic pleasure?' He never speaks of one theory eing nearer to the truth than is another. As no arrangeιent is regarded as final, it is evident that he does not egard the task of scientific research as limited. On the .bsolute theory the scientist apparently aims at finding ut what things are really like. Hence if he claims that a iece of research gives him an absolute truth he has reduced y one the number of absolute truths to be discovered. Vhen all these absolute truths have been discovered the ιsk of scientific research is, on the Absolute theory, finished.

* *Life*, by Gladstone, p. 95. London, 1878.

On the Patterning theory, the task of scientific research is never completed for we cannot be sure that no man will come along with some logically sound, but new way of looking at certain facts. Some at least of these logically sound ways of looking at facts are as much a part of research as is the discovery of facts.

It must be frankly confessed that current language is far better adapted to talking about science from the Absolute Truth point of view. For example, on that view one might say, 'Light *is* electromagnetic waves in a medium called the aether.' The Descriptivist would have to say, 'As a policy of action light can at the present time best be regarded as etc.' In practice this more elaborate phraseology is not usually used and is unnecessary for clarity of thought, except in discussing such questions as the verbal expression of the results of mathematical physics and similar abstruse topics. As the Patterning view is essentially more cautious it may be difficult to adapt language so as to express the necessary reservations with a brevity equal to that of the Absolute Truth phraseology. Whilst brevity may be the soul of wit it is not necessarily the soul of clarity.

The historical example of the way scientists have spoken of light, appears to show that the Description view fits these facts better than does the Absolute. On the Absolute view scientists have at various times said firstly that light consists of streams of particles, secondly that light consists of waves in the aether, and thirdly that no such medium as the aether 'exists' to carry the waves. On the Description view these are regarded as devices for patterning the facts about light phenomena and the serious introduction of the philosophical term 'exists' is forbidden.

Much of what has been said may be expressed alternatively by saying that on the Patterning theory the phraseology of scientific inference falls out of favour. In general the Patternist does not like to talk about what other men think. He cannot observe what other men think, but he can observe what, for example, they say or write. He finds that in practice agreement between different observers is reached only when they make judgments of coincidence.

As he can use this method to observe other men's actions but not their thoughts, he prefers to talk about their actions rather than about their thoughts. This restriction does not apply to himself and he has no hesitation in talking about what he imagines or thinks as well as about what he and others can observe by the coincidence method. He does not use the word 'observe' in speaking of all the things of which he may be conscious. He would not speak of 'observing' his toothache, but would describe some of the things of which he is conscious in terms of feeling, thinking, imagining, wanting, and such like terms. Thought is judged by subsequent action so that the rules of logical thought are regarded as rules of action. For example the logical inconsistency of a man trying to be in two places at the same time is an attempt at a pattern of action, which, however, results in his getting 'hot and bothered,' in his failing to get what he wants or likes and possibly in his getting results which he does not want or like.

Another way of expressing the difference in outlook of the two rival views is to say that the Absolutist is a philosophical scientist who regards man as a creature of pure reason standing apart from the Nature studied by scientists, whilst the Patternist is a non-philosophical scientist who regards man as a part of this Nature, reacting to his internal and external environment. At the end of a piece of research the Absolutist asks what may be inferred, whilst the Patternist asks what may be done. In the present state of knowledge both are continually obliged to use the language of inference in discussing scientific research. The Absolutist uses this language willingly, the Patternist reluctantly.

The student especially interested in impersonal human knowledge will note that whenever scientific inference is seriously discussed, contact has to be made with observations in which agreement between different observers is reached. The Absolutist speaks of these observations as proving something about things in themselves and refers to 'belief' in these things as usable as a basis of action. This is one logically sound way of talking about the actions of research workers, but the Patternist is not necessarily committed to any one

way of talking about action for one logically sound theory does not necessarily exclude another.

The experimental observations of Pavlov and of the Behaviourist* psychologists have rendered vague the boundary between the non-inferential actions called 'reflex' and actions usually explained in terms of some kind of inference. Whilst the actions studied by them seem far removed from the actions used in carrying out research work, their results are indeed coincidence observations and they relate to the behaviour of living things. Some scientists of the highest standing have stated that they see some relation between certain ideas of the quantum theory in physics and the interpretation of human action in terms of the idea of free-will. The scientific relation of human action to the coincidence observations of Pavlov, Watson, and others is far more evident than is its relation to the mental concepts of quantum physics. However, no further attempt will be made in this book to write about research technique in the behaviourist phraseology. This brief reference to a view, other than the inferential, of human action may serve to remind the reader that the results of observation rather than of inference will be used as the basis of argument. In cases of doubt, appeal is made to observable action and not to unobservable inference. Wherever the treatment seems unconventional, the reader is asked to remember that my primary concern is with what men can be observed to do and not with what they think. The appeal to phiiosophy will be regarded as a device of the ruler of the region where cryogenic research can be carried out only with extreme difficulty.

EMOTIONAL OBJECTIONS TO REGARDING SCIENCE AS DESCRIPTION

From one point of view use of the Absolute theory of science may be described as the pursuit of unique explanations. Each scientific explanation must necessarily differ from all

* See Pavlov, *Conditioned Reflexes*, Oxford, 1927; J. B. Watson, *Behaviourism*, London, 1931.

others and must therefore be unique in some sense. On the Absolute theory a unique explanation would be final and would logically exclude any other explanation. The difficulty of excluding mention of the human element can be seen in considering such a simple mechanical problem as the motion of a spot of mud on the rim of a wheel, which is rolling with constant speed along a level road. Seen from the axle of the wheel the mud-spot is moving with uniform speed in circles. Seen from the road the mud-spot is moving with varying speed along an unclosed curve, the cycloid. Lovers of Absolute Truth may want to know how the mud-spot is *really* moving. Surely the mud-spot cannot be moving in circles and also along a cycloid at the same time. What answer can be given? The General Theory of Relativity has reminded us that even in the most abstruse physical problems we do not get away from the observer. Not only must information about the observer be specifically stated, but the results are expressed in terms of what this observer can detect.

Although comparatively new, the idea of regarding science as description has been used in the works of Mach, Kirchoff, Karl Pearson, and Hobson. To those specially interested, it has been known sufficiently long to give ample time for logical or intellectual objections to be raised. The Description theory has been criticized as 'arbitrary.' The term seems to be used to mean 'not excluding other explanations.' In taking account of the human factor in research and developing the Description theory into the Patterning theory (Chapter XI), I found that I was making the Description theory into a still more 'arbitrary' theory. Science as description is made to seem more 'natural' by making all human action seem less reasonable or rather more unreasoned. The statement of 'reasons' given for doing any action cease to be used as explanations. I therefore re-examined all the objections I could find to regarding science as description. This more careful examination revealed only emotional objections. In particular, abandonment of the claim that the scientist's way of looking at things is also the Creator's, seems to be repulsive to some writers.

All criticisms I have found of the Description theory are heavily charged with philosophy and emotion. Science is said to be 'reduced' to 'mere' description. Those who favour the Absolute theory speak as if for them the bottom is knocked out of science if it adopts no creeds and makes no claims to be getting to the bottom of things or to be reaching or approaching Absolute Truth or Inner Reality. As this kind of emotional attitude rather than logical argument is responsible for a good deal of the opposition to the Description theory it is relevant to direct attention to certain facts and hypotheses resulting from the study of human action.

When psychology was freeing itself from the stultifying influence of philosophy one group of workers devoted attention to the actions of men who definitely do not behave like pure reason machines. These workers found with "great frequency" in the insane, grandiose delusions in which the patient claims "to be some exalted personage, or to possess some other attribute which raises him far above the level of his fellows."* Ideas derived from a study of the abnormal actions of the insane or of the mentally ill were later used in studies of more normal action. No brief is here held for the psychoanalytical theories of normal human action, but it is difficult to deny the relevance of these studies to both the claim that science is reaching or approaching the God-like absolute truth and the use of ideas of mathematical physics to rationalize 'belief in' human free-will. From the psychoanalytical point of view 'reducing' science to 'mere' description renders less apparent a relationship with grandiose delusions, although it suggests an analogy with the caution characteristic of 'neurotic' behaviour. If the Patternist has any emotional objection to being called neurotic he has only to devise and use some other logically sound theory of human action. If the emotional objection to being called mildly megalomanic is great, it is only necessary to say that the psychoanalysts have not reached Absolute truth, and who can deny it?

* Hart, *The Psychology of Insanity* p. 31. London, 1929.

THE EXTERNAL WORLD AND ABSOLUTE TRUTH

In dealing with all kinds of human action, including that type called research technique, it is convenient to use the idea of an individual with an external environment which may be called his external world. When an individual thinks about his different kinds of experiences he seems to notice similarities or uniformities in some and not in others. For example, he seems to notice a certain constancy about the rooms in which he lives and works. He finds that the same kinds of things happen whenever he turns on a tap. He may notice different things about it on different occasions, but he finds that whenever he acts in certain ways towards the tap, some of the results which he gets are always the same to him. It is convenient to group these constant experiences together in terms of the idea of an external world or environment which is not altered by thinking about it. Many mental experiences do not have this same constancy for the individual, but as a policy of action man finds that he gets what he wants by doing things which to him seem similar whenever he wants certain results from a tap.

It is customary to explain this kind of action in terms of "belief in the reality or existence of an external world" which is quite independent of human beings. As I see it this is a theory to account for observable action. As no means has yet been found of telling that a logically sound theory which fits the facts is the only theory into which the facts could be fitted, there are no logically sound grounds for insisting that this philosophical theory is the one and only theory explaining a man's action towards, for example, a tap. To insist that a man's actions towards a common object such as a tap 'prove' that he is 'assuming the existence or reality' of the tap is to insist upon using one theory of human action rather than another. As already stated, language is best adapted for talking about human action in terms of inference. For the sake of brevity no attempt will be made to avoid this logically objectionable language when talking of the actions of research workers.

When it is said that a scientist, for example, assumes the reality of an external world or does not believe in chance, the phrases refer to one simple way of explaining some of his observable actions. The particular way chosen does not exclude other ways in which the ideas of belief or faith or inference are not used.

The users of the Absolute theory refer to the idea of an external world as if it were something quite independent of human beings. When they use such a phrase as 'the nature of the physical world' they refer to what some kind of world or universe is really like or would look like if all mankind were dead. The users of the Patterning theory can make no scientific statement whatever about a physical world or a mysterious universe which includes no human beings. For the patternist, science starts from facts. Facts are coincidence observations made by human observers. As a world containing no human beings contains no human observers the patternist can get no facts. He cannot therefore get any scientific classifications, laws, or theories about such a physical world. For him the phrase 'the nature of the physical world' means a collection of physical facts, classifications, laws, and theories, all of which are biological products of that particular kind of animal called man. For the patternist the idea of an external world is then no more than an hypothesis. As he does not use creeds or dogmas, he neither 'assumes the existence' nor believes nor disbelieves in the 'reality' of an external world. For him the idea of an external world is a patterning device especially useful for dealing with those of his experiences which to him seem constant.

CHAPTER III

CHANCE AND THE UNIFORMITY OF NATURE

WHEN a scientist carries out an experiment one day and repeats it under what seem to him to be similar conditions, he usually finds that he can make the same coincidence observations on the two occasions. If the two sets of observations do not agree he will usually be observed to make alterations in what he does until he gets coincidence observations agreeing with those first made. This repetition of coincidence observations whenever the experiment is repeated is regarded as essential before any account of the research is published. Furthermore the details of what has been done must be so described that other research workers may repeat the experiment. If they fail to make the same coincidence observations when following the published instructions, they ask for more details. If they still fail to get repetition when attention is given to the additional details, they usually publish a detailed account of their own results. If the conventional language of inference, assumption, belief, and the like is used, the original result may then be described as 'not proven' or we may say that we do not believe in it. In practice this means that, for example, classifications, laws, and theories would not be altered in accordance with such a result if it did not fit them. If it did fit them, it would not be treated as an example of a fact fitting them.

To explain the agreement between observations made in similar circumstances on different occasions or by different observers a hypothesis called the principle of the Uniformity of Nature is often used. The hypothesis which is probably closely related to that of an external world, discussed in the previous chapter, may be stated: *In the same circumstances the same facts can be observed.* The principle is used as a policy of action by research workers in some such form as: *If in circumstances which seem to the observer to be the same, the same facts (coincidence observations) cannot be re-observed by him, then critical examination of the circumstances will show him that they*

were not the same. The agreement between observations made upon different occasions and by different observers is found in practice. The uniformity of nature is one simple and logically sound hypothesis devised to explain these facts of experience. Many other scientific hypotheses could be devised to explain the facts but none simpler has so far been suggested.

In much of the current popular science writing about physics the writers suggest that the idea of uniformity of nature is unsuited to explain some of the experiments now carried out in physics laboratories. The source of these statements is a misunderstanding of certain passages appearing in such highly technical literature as Dirac's *Quantum Mechanics*. One such passage removed from its context reads: "If the experiment is repeated several times under identical conditions, several different results may be obtained."* The whole passage occurs near the beginning of a book written for the use of mathematical physicists and the barriers of mathematical symbols are found only later in the book. Readers who surmount these barriers understand the highly specialized sense in which such words as 'experiment,' 'states,' and 'observables' are used. The popular science writers do not explain these specialized uses of common words.

The experiment referred to is what may be called a mathematical experiment. It is an experiment with mental concepts which can be carried out with the mind but not with the hands and eyes of a human observer. A simple example of such a mathematical experiment is to be found in any school book of geometry, as when we are told to 'Place the triangle ABC over the triangle DEF so that the equal sides BC and EF coincide.' This is an operation which can be done in thought but not in action. Triangular pieces of paper or cardboard can be fitted one over the other and such action would be a part of a laboratory experiment. But although the ideas of geometry help in action with triangular objects, geometry is not the study of pieces of paper or cardboard and the like.

* Dirac, *Principles of Quantum Mechanics*, p. 10. Oxford, 1930.

The ideas used in mathematical experiments help in taking action in the laboratory, and Dirac has explained in the same book that "The only object of theoretical physics is to calculate results that can be compared with experiments." Again, "The object of quantum mechanics is to extend the domain of questions that can be answered and not to give more detailed answers than can be experimentally verified." In both of these statements the experiments referred to are laboratory experiments carried out by human observers. It is only in these that the idea of the uniformity of nature is used. Too little is known in biology and psychology to justify any scientific statement about the application of the principle of the uniformity of nature to mental concepts or in any experiments which do not give observed coincidences (as defined in this book). Dirac, Heisenberg, and all other mathematical physicists require that any experimental results obtained in a laboratory shall be repeated until they give self-consistent results, before any attempt is made to formulate a mathematical theory to fit them. No living mathematician would attempt to work upon the results of laboratory experiments which when repeated several times "under identical conditions" gave different kinds of results each time.

The idea of the uniformity of nature is used in explaining the observable actions of research workers. In the Description theory of science it is not a creed, but a policy of action. Yesterday I adjusted the size of the slit of a spectrometer by turning a certain screw in a certain way. To-day if I want to make a similar adjustment I shall turn the same screw similarly and shall expect to get the same result. The only possible way at present known to man of proving that I shall get the same result is to try it. The physicist writes textbooks of his subjects, giving experimental methods for measuring physical constants, which methods he expects others will use in the future. The chemist publishes methods of qualitative and quantitative analysis and detailed accounts of methods by which substances have been prepared, in full confidence that in the future the same methods can be used by other chemists to give the same kind of result. To prepare

tissue sections with the microtome the same process may be used to-day as was found satisfactory for similar material yesterday. The autoclave which sterilized apparatus yesterday can to-day be relied upon to sterilize apparatus, if it is worked in the same way as yesterday. The *Radio Times* announces with full confidence that time signals from Greenwich will be radiated on future dates as they have been in the past. In everyday life it is said of some men that they do not seem to benefit from past experience. We mean that they do not seem to recognize as similar circumstances which we judge to be similar, and suppose that conduct which would have given the desired result in the first instance would also give the desired result on the later occasions. The idea of the uniformity of nature is used as much in policies of action in everyday life as in research.

THE UNIFORMITY OF NATURE IN PRACTICE

In discussion with science students I have found that they may have two difficulties when they attempt to compare the application of this hypothesis with their experience in the laboratory and in everyday life.

The first difficulty arises when it is found that in experimenting, even under favourable conditions, with an instrument such as a galvanometer which gives definite readings of some pointer device on a scale, precisely the same readings are never got on closing the electrical circuit several times in succession with no conscious changing of the apparatus between the readings. The galvanometer readings are all very near indeed to the same value, but are not exactly the same on careful observation. The doubt arises whether this principle of the Uniformity of Nature is intended to be applied in the form stated or whether it should not rather be read as: *In precisely the same circumstances very similar things can be observed, or very similar things can usually be observed.*

The second difficulty arises in thinking of the subject of chance. Phenomena such as the tossing of a coin seem to suggest capricious behaviour of an object supposed to be

dead matter. There seems to be no uniformity of behaviour of the coin when tossed.

Before discussing these two difficulties in detail it may be well to note that mature thinkers have similar doubts on the subject as well they may have upon any hypothesis. Apparently the principle of Uniformity of Nature is not in favour with all philosophers. Here are the views of two modern writers. "I am not aware when the Principle was first introduced to an admiring world; but, as it has a pleasantly Victorian flavour about it, I am inclined to conjecture that it made its début at the Exhibition of 1851, when it was awarded a Silver Medal! Comic though the Principle may be in some aspects, it nevertheless needs to be discussed very seriously. The objections to it may be put quite briefly; in the only meaning that can be assigned to it that is not palpably false, it does not fulfil the function it is intended to. The argument in its favour is that when you have said all you can against it you are apparently compelled to acknowledge some assumption about the nature of the external world which comes to very nearly the same thing."* In this and the following extract the writers appear to be using the Absolute Truth theory of science with its characteristic profusion of philosophical terms.

There appears to be no doubt about the meaning of the following view which specifically mentions both chance and the Uniformity of Nature. "At the same time, there is *prima facie* evidence enough of disorder and chance in the universe. Not only are there vast regions of fact in which, as yet, no order has been discovered, but even among the orderly facts or events there are apparently elements of disorder. The individual members of any class of phenomena, for example, usually show considerable deviation from the type . . . and the most careful measurements of changes subject to natural laws almost always show deviations from one to another and from the adopted, or so-called true, value. These deviations (or 'errors' as they are often called) cannot be entirely explained away on the ground of human

* A. D. Ritchie, *Scientific Method: An Inquiry into the Character and Validity of Natural Laws*, pp. 91–2. London, 1923.

incompetence. They seem to point rather to an element of lawlessness (originality or spontaneity, if you like) in the facts or events themselves. . . . If we are to use the familiar expression 'Uniformity of Nature' to describe the general character of nature, as just discussed, it must not be taken to mean more than that there are laws or uniform connections in nature, or in many phenomena of nature; and it must not be taken to mean that such laws are absolutely rigid, or that all the phenomena of nature are subject to laws."*

Now let us return to the practical examples. The reader who is not a physicist will here have to substitute an example taken from his own experience. From the nature of the argument it will be seen that technical knowledge of each individual example is essential. Suppose that a ballistic galvanometer is in use to compare the electrical capacities of two condensers and that the instrument is arranged so that its mirror reflects a light signal image onto a graduated scale. Part of the method consists in charging up one of the condensers to the electrical potential of a constant voltage battery and then in discharging it through the galvanometer. The deflection produced on the scale is then read, reliable observations being obtained by first trying several times until the observer knows upon what mark on the scale attention must be fixed. Now the puzzling thing about the observations is that even after the constants of the apparatus have been adjusted so that all parts are working in the way they were specially designed to work, the galvanometer readings differ very slightly each time the experiment is repeated. If necessary the readings must be magnified until some slight difference in their values is observable. One logically sound interpretation of this phenomenon is that Nature behaves in nearly the same way each time the galvanometer reading is taken but that the behaviour is rather like that of a good bowler in cricket. He can be relied upon to throw the ball nearly the same way each time he tries, but he cannot succeed in throwing it in quite the same way every time. Nature tries to give the

* A. Wolf, *Text Book of Logic*, pp. 276–7. London, 1930.

same flick to the galvanometer, but does not quite succeed. The Uniformity of Nature is nearly, but not quite uniform.

Another point of view is that the galvanometer does not give the same deflection each time because the conditions of the experiment are not precisely the same each time it is repeated, and this even although a serious attempt has been made to keep them the same. The reader may have noticed the reference to a condenser charged up to a constant potential battery. Is this constant potential battery beyond reproach? Of course on examination it may well prove to be not quite constant. If several readings are taken fairly close together in time, the condition of the battery will be similar, but the air and the coil inside the galvanometer may not be quite at rest before each reading is taken and the suspension system of the galvanometer will differ each time. In any case the first time a current is sent through the circuit the electrical resistances have been warmed up a little, so altering their value. This is repeated each time a galvanometer reading is taken. If long intervals occur between the observations the condition of the suspension system of the galvanometer, the temperature of the laboratory, the condition of the battery, the leakage conditions of the condenser, the traffic conditions affecting the vibration of the galvanometer supports may all be changing. In brief there is abundant evidence that the circumstances are *not* precisely the same. Adoption of the principle of the Uniformity of Nature would not therefore lead us to expect the same galvanometer deflection in the repeated experiments.

This is only one example of an experiment in which at first sight the results appear to fit better the idea of a non-uniformity in behaviour of Nature. On closer examination it is clearly not essential to postulate any non-uniformity.

Now this example is typical. It illustrates that to get uniform results not only must one attempt to make the initial and boundary conditions of the phenomenon the same. Achievement of these ideal conditions is also essential if precisely the same circumstances are to be observed as leading to precisely the same result. Usually a scientist can have sufficient knowledge and experimental skill to do this

only in his own particular science. For the non-experimental scientist or for the philosopher it is still more difficult even to conceive what would be likely sources of a slight deviation of experimental observations from some mean value to which they all closely approximate.

CHANCE

As a result of critical examination of a representative example of the first type of difficulty it has been found unnecessary to abandon the Uniformity of Nature principle in circumstances where its use seemed at first sight contrary to observed facts. The second type of difficulty is in the phenomena of chance. As the derivation of the word suggests, good examples are found in considering the fall of tossed dice or a coin. The coin is flicked and thrown upwards so that it rotates whilst moving through the air. When it falls and comes to rest it must lie with either heads or tails uppermost. Whichever of these two possible results is obtained is said to be a matter of chance. It is well known from general experience that no man can be sure of correctly foretelling which of these results is obtained. In general the use of the word chance implies that the result cannot be foretold because the coin behaves in an arbitrary or capricious way, not subject to any observable regularity. This is commonly regarded as an example of non-uniformity of nature.

When the physics of the phenomenon is examined it is found that if, say, a steel ball is thrown upwards with the same initial speed, then every time the ball rises to the same height and the same time is taken to reach that height and to fall again from that height. Many experiments have been performed in which it has always been found that the translation of the 'centre of mass' and the rotation of a moving body can be regarded as proceeding independently. It is therefore reasonable to study the rotation of the coin and the movement of its centre of mass separately. If the centre of mass of the coin is given the same initial impulse it will always rise to the same height above the point of projection and it will always take the same time to reach

that height and to fall again. Also if it is given the same impulsive 'couple' it will always rotate with the same speed. The number of rotations in a given time will be always the same and could readily be measured, for example, by use of a slow-motion cinema photograph of the coin in motion. Hence the coin given the same total impulse would always take the same time to complete the toss, it would always make the same number of half revolutions, and would always fall with the same side uppermost if it began its toss with the same side uppermost each time.

Now let us look at the physiological and psychological parts of the phenomenon. If the coin has to be given its initial impulses by a flick of the fingers, what considerations would lead us to think that any human being could apply the same initial impulse to the coin on two or more occasions? I can think of none. The pianist has to give to the piano keys timed impulses similar to those needed to toss coins, but years of practice are necessary before this can be done with sufficient control to play a scale uniformly. The control necessary to play a so-called 'pearly' scale is not attained by even all virtuosi. Even those few who do attain it do so only after years of practice. In this analogy the test of uniform loudness in the resulting piano tone is used to judge the accuracy of control of a flick of the fingers such as would be used in tossing a coin. Even where a uniformly loud tone is heard from a single repeated note, our hearing can only judge uniform loudness very approximately. Considerable variation in loudness would have to be present before any variation could be detected by ear. If when a coin is held ready to be tossed the fingers are watched, unintended motion can readily be observed before any attempt is made to toss the coin. The motion of the hand is affected by breathing. Without thinking of the possibility of sending uniform nervous impulses there is sufficient evidence to justify the view that a coin could never consciously be given the same impulse twice by any man. If the impulse given to the coin cannot be controlled, why expect the 'fall of the coin' to be under control?

To complete this detailed examination of an alleged case

of non-uniformity in Nature one other feature, common to all phenomena usually described as affairs of pure chance, may be noted. This is the importance attached to a small difference in a result. There is little difficulty in justifying the expectation that a tossed coin allowed to fall on the floor will come to rest with one face in contact with the floor. This would be regarded as a satisfactory solution of the mechanical problem. However, when thinking of chance great importance is attached to knowing which face will lie uppermost. When using a ballistic galvanometer we noted, but did not regard as of great importance to the measurement, that sometimes the deflection was 70 mm. and sometimes 71 mm. With the tossed coin the result with seventy half-rotations of the coin is regarded as quite different from the result with seventy-one half-rotations. One may give heads, the other tails. A man with good muscular co-ordination can verify for himself that he can control to one part in seventy, the impulse given to a body thrown upwards, by throwing a ball indoors so that it just touches the ceiling. If he gives the ball too great a velocity it returns quicker and if too low a velocity it fails to hit the ceiling. A fundamental property of all phenomena of chance is then that a small variation in a final result, which in other circumstances would be regarded as unimportant, is in *chance* regarded as of great importance.

Other examples of chance which can be worked out similarly to the tossed coin are the tossed dice, the dealing of playing cards, and the horse race. If a horse wins a race by a neck it may in the same time run a distance only one part in two thousand greater than that run by the horse which does not win the race. In the laboratory the physicist often has to be content with repeating a result to within one part in a hundred. The same man who attaches great importance to the 'neck' of the horse race would attach not the slightest importance to a variation of one part in two thousand in the amount of champagne in the glass in which he celebrates the win.

The second essential feature of an affair of chance is inability to control the initial conditions with sufficient

accuracy to produce the desired final result. Those phenomena which are especially identified with chance are used, not to derive knowledge, but to make a decision. A coin is normally tossed, not to see whether it falls heads or tails, but to decide upon some mode of action. It is sufficient for the purpose that no man can foretell the result of the toss, no matter how much knowledge he may have, because his knowledge will not give him power to control the result. If he were suspected to have abnormally good power of muscular control, he would not be allowed to toss the coin. The research worker knows only too well that much of his time and effort in the laboratory are spent in getting sufficient control over the conditions of the material and apparatus of his experiments. Phenomena of chance provide evidence that if we do not start with precisely the same initial conditions we do not in actual experience get the same result. In these facts there is nothing to make unreasonable a use of the uniformity of Nature principle.

I would very strongly emphasize that although the whole of this discussion deals with facts which fit the idea of the uniformity of nature, the facts prove neither the 'truth' of the principle nor that uniformity 'exists' in nature, nor any other philosophical idea. The proof of the pudding is in the eating, and it is solely with this sense of the word proof that the research worker is concerned. The principle has been found to 'work' when used as a policy of action in dealing with objects and events or more precisely when dealing with facts (coincidence observations).

Whether or no the principle will be found useful in dealing with mental concepts, which cannot be observed by the coincidence method, experience alone can show.

PROBABILITY

It may be noted that the sense in which the word chance has been used is not that of statistical treatments. In reading any of the literature of scientific research in which the words chance or probability are used it is essential for clarity of

thought to distinguish between at least two meanings, the one relating to statistical facts and the other relating to such biological factors as 'expectancy' and 'belief.'

To get scientific knowledge of chance phenomena such as coin tossing, observations must first be made by human beings using actual coins. It is found in practice that the total results of an experiment in which a coin is tossed, say ten thousand times, are similar whenever the experiment is repeated. This type of experiment gives the physical basis of the mathematical theory of probability. If any claim is made that the mathematical theory of probability deals also with expectancy or with 'what it is rational to believe' then properties of human beings are very definitely involved, and the critical reader will examine with great care what biological and psychological facts are considered in the mathematical theory of probability. In popular science writing on electron physics the word probability has been used, like the word Mesopotamia in other circumstances, as if it were specially blessed.

A crucial test can be made of the part played in mathematical treatments of any probability subject, by the everyday ideas associated with probability. First choose some word such as 'obability' which has no rational meaning for the user, and substitute it for the word 'probability' in any mathematical treatment wherever the term is found. If an attempt is now made to understand the mathematics, it is evident that only such meaning as can be found from the equations, can be attached to the term 'obability.' Unless the equations express such ideas as 'likely to occur' or 'rational to believe in' we shall be unable to attach to 'obability,' those ideas which can scarcely be avoided when using the word 'probability.' The surprising thing is that the mathematics of probability can, if it is understood at all, be understood just as well without using the everyday word 'probability' with all its common associations, as with the word. Whether this is because the equations of the theory of mathematical probability express, for the mathematician, the ideas commonly associated with probability in everyday life, or whether it is that the mathematics of probability

deals rather with statistics, the reader is left to decide. It is significant that the mathematicians themselves are not at present satisfied with the theory. Under which curse is the subject lying, the indeterminacy of verbal meaning or the neglect of biological factors in a subject which concerns man and his environment?

CHAPTER IV

TWO LIMITATIONS: THE SHOULD-OUGHT MECHANISM AND ASSESSMENT OF VALUE

BEFORE a scientist starts the regular practice of scientific research he has usually lived twenty or more years. In this time he has been subject to the influence of an ever-changing environment. He has formed many habits of action and of thought, or to use another phraseology he has had many reflexes or responses 'conditioned.' Research is only one of his many types of action. Which of these habits or 'conditionings' are of service to him in research and which are not? There appear to be only two which are dangerous or useless as devices of research technique. For convenience of reference the first will be called the *Should-Ought Mechanism*. (The word 'mechanism' is used in a similar sense in this phrase as in the psychologist's technical term 'Defence Mechanism.') The second will be called *Assessment of Value*.

Before defining these devices, let us think of two ways in which an adult gets ideas of, or reacts to, a certain change in his environment. Suppose that he happens to be in an orchard in autumn. He sees an apple fall off one tree and from a tree which has already lost its fruit he may see a leaf slowly flutter to the ground. If he has picked up apples and leaves he will have noted kinesthetic differences, that is, he will have felt different muscular sensations on picking up a basket of apples and a basket of leaves. He may be sufficiently sophisticated to generalize these differences into the idea of weight. We are concerned here with only two ways of reacting to the particular circumstances of noticing an apple falling quickly and a leaf falling slowly to the ground. Noticing that those things at which he has to tug harder to lift or move them fall more quickly, he may generalize and say that the heavier a thing is the quicker it falls. At this stage there is a very striking contrast in the subsequent reactions of the scientist and of the typical philosopher. The scientist begins either to apply controlled

observation, the technique of experiment, or he seeks information from those who have applied this technique. An apple would be cut in half and the fall of the two halves compared. One of these halves would be divided again and the fall of one-quarter compared with the fall of one-half and so on. The fall would be studied in vertical tubes from which much of the air had been pumped. In other words to find out how things would appear to behave on falling, the scientist uses controlled observation. He uses what aid he can get from his sense organs.

The man of the more philosophical type reacts differently. He confines his attention rather to thinking about things. He may be equally familiar with the sense data of the scientist, but he confines his interest to those of his ideas which he can form from thinking about the sense data, but which do not lead to a search for subsequent sense data. The scientist's thinking about sense data or facts is followed by his seeking new facts. The non-scientific thinker's contemplation is followed by the making of statements which cannot from their very nature be 'technically verified' (see Chapter XIV).

The *Should-Ought Mechanism* and *Assessment of Value* are two examples of this latter type of thinking or of reaction to one's environment.

THE SHOULD-OUGHT MECHANISM

Whenever a statement is made in which the words *should* or *ought* are used in an absolute sense, or whenever the statement could be put in that form without altering its meaning, we shall say that the Should-Ought Mechanism has been applied. This device used by all leads us to say what things should or ought to be, or to appear to be. We say also what men should or ought to do or to say or even to think. To illustrate an application of the Should-Ought Mechanism an example will be chosen dealing with things in which observations can also be made very easily. Surprisingly enough the statement in which the Should-Ought Mechanism is applied is actually based upon reliable observations. This

rather than an ethical subject was carefully chosen as one unlikely to arouse the reader's emotional reactions and is suggested by a notice to be found upon some of the older weighing machines on the platforms of railway stations. The tables are headed "What you should weigh if in health" and a man forty years of age say, of height 5 feet 10 inches is told that he should weigh say, 12 stones 8 lb. Now where did this information come from? Some of the machines give

Weight in Pounds	Height in Inches			Total
	66	67	68	
117	5	2	—	7
124	6	5	—	11
131	12	14	7	33
138	19	25	8	52
145	33	23	22	78
152	20	21	16	57
159	14	23	26	63
166	14	18	20	52
173	12	7	17	36
180	5	2	12	19
187	1	4	7	12
194	1	5	2	8
201	—	2	4	6
Total ..	142	151	141	434

a reference to certain anthropometric tables which I have been unable to examine. The results later quoted are, however, of the same type and are equally reliable. I understand that it is not usual to publish the detailed measurements of anthropometric surveys, but that these are usually filed in some research institution such as the Galton Laboratory of University College, London. The heights and weights of healthy men arranged according to age can readily be subjected to coincidence observation so that if it be said that a healthy man whose age lies between twenty-five and twenty-nine years, and whose height lies between

66 and 68 inches, 'should' weigh 145 lb., the statement can be compared with what can be observed to occur. The table given on page 60 is typical. It refers to British males age twenty-five to twenty-nine years and gives the distribution in weight in 7 lb. units for each inch of height. The values were kindly supplied by Professor R. A. Fisher and are in the manuscript of Dr. Elderton. They were compiled from the Galton collection at University College, London, by Professor Rugar of Columbia University, New York. It is seen that when 434 men were examined all of whom 'should' have weighed 145 lb., only 78 of them were of that weight. If those whose weight lay between 138 and 152 lb. be included, still only 187 of them reach this postulated perfection. If the recalcitrant ones be given another chance of salvation and their weights be permitted to lie between 131 and 159 lb., still 151 fall from grace. Whenever the Should-Ought mechanism is applied to material which can also be subjected to observation this discordance is apt to be found. I have discussed the problem of the variation of biological material with biologists and find the example typical.* All rats may have four legs each, but to find two rats with the same leg dimensions is extremely difficult or rare.

THE MEANING OF *NORMAL* AND *NATURAL* IN SCIENCE

In discussing the statements found on some weighing machines data unpublished in detail had to be used. More detailed data of height and age only of men have been published. Let us look at results obtained in measurements of height of some Scottish soldiers aged nineteen years. The height is given in millimetres and the results are given in 50 mm. groupings. The numbers in brackets are the numbers of soldiers of each of the indicated statures: 1,495(3), 1,545(21), 1,595(99), 1,645(190), 1,695(250), 1,745(193), 1,795(84), 1,845(16), 1,895(4).† It will be seen that of the

* For examples of the 'scatter' of measurements made upon human reaction to drugs (see Fig. 29, p. 300 and Fig. 30, p. 301).

† J. F. Tocher, *Anthropometric Observations*, Henderson Trust Reports, iii, p. 159. Edinburgh, 1924.

860 men measured those of the largest group were of stature about 1,695 mm. and more than half were between the range 1,645–1,745 mm. There were, however, some who measured more than this and some less. If the stature range of 1,645–1,745 mm. be called the normal height for Scottish soldiers nineteen years of age and the other values abnormal heights, what meanings shall be attached to the words normal and abnormal? In science it is understood that 'normal' expresses majority or large number of a group and 'abnormal' means minority. It is unscientific to say that any individual Scottish soldier who is nineteen years of age should be of stature about 1,645–1,745 mm. If the facts warrant it the statements that the majority measure that height or, more cautiously, that the majority measured are of that stature are scientifically correct. The statements that the four who measured about 1,895 mm. were too tall and the three who measured 1,495 mm. were too short are in unscientific form. The scientific forms are that only very few of Scottish soldiers nineteen years of age are as tall as 1,895 mm., and only very few as short as 1,495 mm. We cannot remain scientific and say that something has gone wrong with the growth of these few soldiers. Abnormal here means something like minority, *not* a mistake on the part of Nature. It is easy to understand that men must not be taller or shorter than certain heights in order to be admitted into the army. If this regulation were not enforced many different sizes of uniform would have to be provided and soldiers could not be used for decorative purposes so well, for the very tall and very short ones would spoil the pattern of the uniform arrangements of ceremonial occasions unless their positions were carefully arranged in the ranks. Then, too, in war time, the heads of the very tall ones might stick over the tops of the protecting trenches or the very short ones would not be able to reach high enough to shoot. There are many reasons one can understand for enforcing a regulation about the height of soldiers. No objection is here raised to using 'must' in an absolute sense where the thing can be enforced. The scientific objection is to using 'should' and 'ought' in an absolute sense. Only observed

coincidences can help us to tell what scientific views can be held about what Nature should be. What Nature should be is what it is.

The Should-Ought Mechanism has no place whatever in research technique and its complete and unconditional abandonment is one of the foundation stones of science. It is premature to worry about experiments and the working out of the technique of experiment in general until a man has become dissatisfied with the Should-Ought Mechanism. Even if the reader is or is likely to become a scientific genius he would still find it much easier to say what Nature should be like rather than to bother trying to find how some part of it appears to behave.

In everyday life it is quite common to use the words 'normal' and 'natural' with an additional meaning to express approval, and 'abnormal' and 'unnatural' to express disapproval. A freak of nature is referred to as unnatural and as if it is something in which Nature has got muddled and done something which ought not to have been done.

SOCIAL APPLICATIONS OF SCIENCE

If the inability of scientific research to determine 'shouldness' or 'naturalness' is not frankly acknowledged, there is grave danger that the results of biological and psychological research may, by some, be so expressed that they appear to indicate that mankind should or should not adopt some particular kind of action. The technique of scientific research is well adapted to determining the effects of adoption of some policy of action, but it is powerless to determine anything whatever about the 'shouldness' or otherwise of the action. There is little doubt that in the future the biological sciences will be developed to a stage similar to that of physics at the present time. The non-scientist will then be well advised to examine with great care the non-technical statements of biologists interested in social, political, or ethical questions.

To many research workers much of what has been said may appear too 'obvious' for comment. But false impres-

sions of the limits of research technique are frequently given to the non-scientist by the public statements of some research workers of the highest scientific standing who happen also to be interested in political, social, or ethical questions. Some of these scientists have even urged that purely scientific societies, such as the Royal Society, should officially urge governments to carry out certain actions presumably on the basis that scientific knowledge shows the 'rightness' of the action. Any public statements made by these individual scientists are assumed by most non-scientists to represent a scientific point of view upon the particular problem.

Now one of the most extensive uses of the Should-Ought Mechanism is in the field of ethics or morality. If it be accepted that the complete and unconditional abandonment of the Should-Ought Mechanism is one of the fundamentals of research technique, it follows that whenever a scientist is making a statement of what is, for example, moral or immoral, he is not speaking as a scientist. I do not wish to imply that such a scientist is not sincere. Rather the opposite, for he tends to lose scientific caste by having anything to do publicly with political, social, or ethical questions. I wish to emphasize that in reaching the conclusion that some type of action should or should not be done, or is either moral or immoral, he is unscientific. That is to say, he is not using the same kind of actions or thinking which he has to use to carry out his research work.

If a scientist is asked what should be done in any particular circumstances he cannot give a scientific answer. If, however, he is asked what should be done *in order to* produce some stated result then he may be able to give a scientific answer. If the necessary facts are available he can answer at once. If they are not available then the technique of research is well adapted to get them in a time depending upon such factors as the nature of the problem, the research facilities available and the mental ability of the scientist.

This statement that the technique of scientific research contains nothing whatever which enables its user to determine political, social, or moral laws or codes must not be interpreted to mean that the subject matter is outside the

scope of scientific research. For example, although a scientist would be lacking in worldly wisdom, he would be speaking as a scientist if, when he was offered a code of morals formulated by a man in a state of food or sex starvation, he were to offer in exchange an accurate statement of the facts (coincidence observations) and theories relating to the differences in behaviour of man in this abnormal and in a normal state. The reader will be familiar with one example in the difference in outlook of a man before and after a good dinner. Nevertheless all the facts and theories would be only a scientific explanation of the actions of the formulator of the moral codes; they would have no scientific connection with the idea of obligation implied in the codes. That they may or may not influence the subsequent actions of the scientist, is outside the scope of the present discussion.

Another example in which clarity of thought demands a full realization of the limits of scientific method, is in the study of the distribution about a mean value of individual differences in all manner of living things. Earlier in this chapter an example has been given of the way in which the heights of men having certain common characteristics are not constant, but vary widely. It was seen that they could be divided into groups, one of which contained more than any of the other groups taken singly. About this group the others are equally distributed above and below. The application of scientific method takes us no further than this, and gives no information whatever about what those outside the normal group should be. This simple example is relevant to the discussion of a certain point of view upon the help which scientific research can give towards solving some of the problems of civilization. Some of the more serious laymen have noted that the biological and psychological sciences have to deal with objects and phenomena which appear much more complex to the human mind than do the subject matter of physics and chemistry. They say that when the biological sciences have been developed as far as have the so-called exact sciences, then the knowledge will show us what ought to be done in dealing with the

problems of civilization. The consideration of the simple example given earlier in the chapter shows that such an idea is not justified by the facts. The more the biological sciences are developed the more will it be possible for the biologists to tell us what to do *in order to* produce a specified result, but with the current research technique it will not be possible to tell whether or no the means should be applied. At present the biochemist can tell us how to make and apply thyroxin to the young cretin child so that its growth will be normal or almost normal, but if he speaks solely as a scientist he cannot tell us that we should or should not apply the thyroxin. Present-day research technique cannot determine 'shouldness' or 'rightness.'

Note on the Scientific Form of Statement.—In view of the fact that the public statements of some scientists include statements about what should be done, it may be of interest to note the characteristic scientific form of statement.

In order that any statement whatever may be scientific it must express or imply some qualification. Every scientific statement contains or implies such phrases as *In order that*, *Provided that*, *For the purpose of*, *To be consistent with the law or theory of*, and similar qualifying phrases. Frequently the implications are left unstated in order to save time, energy, and space, but there is always a danger that when the omission is made the statement may be misunderstood by others than the specialist in whose sphere the statement lies. It is quite possible to make statements in scientific form even if their content is quite outside the range of sciences. For example a church may issue a statement that 'In order to be a Christian a man should do, etc. . . .' The should-ought mechanism is not here in use. If, however, the statement is made in the form 'All men should do, etc. . . .,' and if the phrase 'in order to be Christian' is not implied then the should-ought mechanism is being applied and the statement is an unscientific form.

The preceding paragraph is intended solely for the reader interested in the possible application of research technique to problems of civilization. On first thoughts it seems as if

the insistence in this chapter that current practice in scientific research contains no means whatever of determining 'shouldness,' is logically inconsistent with the suggestion that research technique may help in solving problems of civilization. If a man suggests a means of helping in the solution of these problems it is customary to ask him what he thinks 'should' be done. At the same time it would be generally agreed that the problems are problems of human action not of human thought. (By action I mean action which can be observed by the coincidence method as explained in Chapter VI. In this sense by definition, thought, is not a form of human action; speech and writing are.) That is to say, problems of civilization will be solved or will disappear as a result of what men do. Beyond this point the agreement so far reached is discontinued, for some say that the things men do are based upon thought and follow from application of pure reason, the Should-Ought mechanism and assessment of value. Other men say no more than that these devices often go with human action. To say that problems of civilization can be solved only by men first determining the 'shouldness' of various actions is to imply either (i) that the 'shouldness' of an action is something which can be determined by the technique of experiment, using the coincidence method which establishes the 'shouldness' of the action as a fact, or (ii) that if 'shouldness' cannot be determined by the coincidence method, then it is a mental concept essential to the only theory of human action which fits the facts. The first implication is contrary to the findings of experiment, and the second ignores the fact that whilst no satisfactory theory of human action has yet been formulated, several of the current theories do not include the postulate that human action is based upon initial determination of 'shouldness.'

The contents of this chapter may so far be summarized as follows. The Should-Ought Mechanism, which is widely used in everyday life by both scientists and non-scientists, is completely abandoned in the practice of scientific research. Research technique is not for this reason necessarily powerless to help in the solving or removal of problems of

civilization, because these are problems of human action and the 'shouldness' of an action is neither a fact nor an essential mental concept initiating human action.

ASSESSMENT OF VALUE

The second of the limitations of scientific method relates also to types of statement which cannot be 'technically verified.' The limitation is the abandonment in the practice of scientific research of assessment of value. By assessment of value I mean the arranging of objects or events or actions in some kind of order of merit or value. Typical uses of the device are seen in statements that one thing is better or more important than another. This section is not concerned with such problems as determining whether some thing has ethical, aesthetic, logical, religious, or economic value, to mention only a few of the possible types of value. Neither is it concerned with such problems as comparing one kind of value such as aesthetic with another kind of value such as moral. The concern is with first choosing any one kind of value, such as ethical, and then saying that, for example, one thing is better than another in the same type of values.

The relevance in a study of research technique of a brief discussion of the use of the device is twofold. Firstly, scientists are as much concerned with arrangements of facts as with isolated facts and assessment of value is one device which can be used to arrange facts in some kind of order. Secondly, research technique is here regarded as a type of human action and the making of statements of assessed values is one of the most widely practised actions amongst civilized adults. The conversation of everyday life, the speaking and writing of men capable of the simplest or the most complex of behaviour is full of assessments of value. All men continually assess values throughout life. If then the device can be used in the practice of research we shall have added to the judgment of coincidence (see Chapter VI) a second research device which all men are fully able to use.

It must be at once admitted that the language of valuation is used as much in the laboratory as outside. The analysis of one particular example will, however, suffice to show that when such words as good, better, or important are used in scientific research they are used for brevity (as often happens in everyday life) and not to express assessment of some kind of value. Suppose that in the course of a piece of research work an organic chemist has to prepare some rare chemical compound which cannot readily be bought. He may refer to standard reference books of methods of preparation and find that since the original preparation of the compound several different methods of preparation have become available. In speaking loosely one might say that naturally he has to make a choice and will choose the best method. Perhaps one of the newer methods will be better than that used by the man who first prepared the compound. Here then is a man apparently using in research the device of assessment of value. As this appears to be in direct conflict with my view that assessment of value is not used as a tool of research, the facts must be examined in more detail.

It may be found that one method of preparation may start from more expensive material than another. A second method may take less time than another or it may give a greater yield or it may give a product less likely than another to have traces of some impurity which would adversely affect the subsequent experiment. A third method may need a piece of apparatus not available in the laboratory in which the man is working. A fourth method may need almost constant attention whilst another may take a larger total time, but might occupy less of the man's time so that the preparation might be combined with demonstrating in a teaching laboratory. It will be seen that the problem starts with a want and that each of the considerations which may affect the choice of method of fulfilling that want, involves judgment of coincidence, not of values. The judgments include counting money, measuring time or mass of material with the aid of a chemical balance, or judging the presence or absence of a piece of apparatus. It is precisely these kinds

of factors which research workers frequently have to consider very carefully before deciding what to do next in the laboratory. If one chemist asked a colleague what was the best method of preparing some compound, a scientific answer could not be given. The colleague would almost certainly be observed to ask, 'Do you want a large yield?' or 'Do you want a product free from a certain impurity?' or some similar question. This example of the use of one of the words of valuation, 'best,' is typical of all scientific uses of the word and of other valuation terms. (Many examples from everyday life can be similarly analysed and are similarly not assessments of value.)

It may seem strange to devote a chapter in a book on research technique to two devices which are not used in the practice of research. But it must be remembered that the whole book is based on the idea that whatever else it may be, research is a form of human action. The first thing needed for research is at least one human being. Human beings must be accepted as they are made and all men are so made that, long before they are old enough to do research, they have become fully accustomed to apply the Should-Ought Mechanism and assessment of value. They continue to do so throughout life whether or no they are ever scientists. Therefore these two corner-stones of research consist of only a temporary abandonment of two devices which are normally in frequent use. That the abandonment is only temporary is shown by the fact that when a piece of research work is completed, research workers apply assessment of value to it. The abandonment is only in the actual practice of research. It would appear that whilst the Should-Ought Mechanism is perhaps only a rather vague way of expressing certain of our wants, assessment of value is a vital necessity of mental health. I personally regard it as a defence (of superiority) mechanism, for it usually seems to happen that by hook or by crook all mentally healthy men manage to arrange that their assessments of value fit their wants and likes very nicely indeed. A man's set of values is usually a very accurate guide to his personal qualities, possessions, and associations.

Unlike such a device as the technique of experiment which is widely, but not wholly, inapplicable outside the laboratory, the two corner-stones of research here discussed are applicable to any subjects whatever. The rest of the chapter will be devoted to analysis of those fundamental qualities of assessment of value which make it an unsuitable research tool.

LOGICAL INCONSISTENCY OF SOME TYPICAL ASSESSMENTS OF VALUE

The fundamental reason for the rejection of assessment of value as a tool in research is that scientists want human impersonal knowledge, whilst assessments of value give something highly personal. Scientists want human but impersonal relationships between man and his environment. Value assessment gives human but personal relationships between man and his environment. Those philosophers who do not regard values as 'logically primitive' and therefore indefinable concepts, seem to regard them as means of organization of human experiences which are, or can be, common to all individuals.

Throughout this book attention is frequently given to the agreement or failure to reach agreement in the use of any device which may be used as a tool of research. If assessment of value be considered from this point of view disagreement between different individuals or groups of assessors are invariably found. Consider only a few examples of, say, ethical values. According to Professor Sumner, the following actions have, at one time or another, been considered the 'normal' and unemotional way of acting. "One woman could have many husbands; one man many wives; the offspring could be killed in times of famine; human flesh could be eaten; sacrifice of offspring could be made to appease deities; you could lend your wife to your neighbour or guest; the wife was acting properly when she burned herself on the pyre that consumed her husband's body."* Nowadays these acts would all be rated as lower in ethical value than formerly. There are, however, no reasonable

* Watson, *Behaviorism*, 2nd edn., p. 146. London, 1931.

grounds for supposing that the physical or physiological results of these actions would be any different now from formerly. The facts (coincidence observations) remain unchanged, but their ethical or other valuation differs with time.

If the time factor and the stage of civilization be kept unchanged there is still failure to get agreement between different assessors. Think of the varying assessments of the ethical value of one man killing another human being when the act can be described by any one of the terms, accidental killing, homicide, manslaughter, murder, assassination, execution, judicial murder, regicide, 'killing in warfare,' or infanticide. In all these acts the physical and physiological results upon the human killed would be the same, but the moral valuations would be different.

The reaching of different results is not, however, in itself enough to condemn a device as a tool of research. One test which must, however, be passed by every device before it can be accepted for use in scientific research is that when applied to the same material it does not yield logical inconsistency. If attention be further restricted to, say, the assassination of a man, it may well be found that one group of men may regard the assassin as a deliverer of his fellow men, rating his act as high in ethical value, whilst another group of men may regard him as a murderer whose act they rate low in ethical value. If a valuation, in this case an ethical valuation, is independent of the individual assessor, then when valuation is applied to the same act it yields the result that a certain action is of both high and low value. Valuation here yields a logical inconsistency.

Is it only in ethical or moral values that assessment of values yields self-contradictory results? The answer to this question is undoubtedly 'No.' In aesthetic values it is frequently found that what one man calls beautiful another calls 'freakish,' or ugly. In religious values it is doubtful if there is any action which a man may wish to carry out which could not be called of either high or low value according to the particular religion selected. In highly civilized peoples it is not always necessary even to change

one's religion, for the addition, to some condemned action, of another action involving economic values can result in changing the religious value of the original action. In warfare between civilized nations in which the state religion is Christianity the majority of religious bodies in either of the two states assess high in value the warlike actions of its own state. Each says that its own state's cause is righteous, and the other state's cause unrighteous. That is to say application of assessment of values in dealing with religious values here leads not only to different results but also to self-contradictory results.

It seems reasonable to adopt the hypothesis that values depend upon the valuer, and that assessment of value does not lead to self-contradictory results because in valuation each man is a law unto himself. On this hypothesis such agreement as is reached by different valuers would be explained by the limitation of the number of different values which can be expressed in human language. If, however, the article on Values in the *Encyclopaedia Britannica* may be taken as representative of the current views of philosophers then the first part of this hypothesis would be rejected by all philosophers. It is stated that although there is much disagreement between philosophers on the subject of values "There is a substantial agreement that values are not subjective in the sense that they are merely matters of opinion and exist only for the persons who appreciate or feel them."

As the subject of values is usually regarded as a branch of philosophy and is therefore apt to become entangled in metaphysical nets it may help clarity of thought to summarize such results as seem to be relevant to the study of research technique. I wish to warn the general reader that the statements here made would appear to differ from those of most philosophers who have written on values. The classical writers on scientific method do not treat of the subject. The first part of the following statement by Clerk Maxwell in his British Association address (1870) on the "Relation of Mathematics and Physics" is, however, consistent with the present treatment. "It was a great step in science when men became convinced that, in order to

understand the nature of things they must begin by asking, not whether a thing is good or bad, noxious or beneficial, but of what kind it is? and how much is there of it? Quality and quantity were then first recognized as the primary features to be observed in scientific inquiry."

The summarized results are as follows. Assessment of value is a device by which facts may be grouped together or ordered. It is a device used throughout life by all, scientist or non-scientist, both before and after the practice of research. There is, however, no subject to which application of the device has so far been made in which general agreement between different assessors has been reached. When it is applied to any type of value, examples can be found in which logically inconsistent results are reached by different assessors even when they are of the same stage of civilization and when they make their assessments about the same subject matter and at the same time. The fundamental ground of rejection of the device as a tool in research is its liability to give logically inconsistent results when applied to the same subject matter by different men. After application of either the Should-Ought mechanism or assessment of value, *Cosi è, se vi pare.*

GETTING SCIENTIFIC FACTS

"It is always safe . . . to distinguish as much as is in our power, fact from theory; the experience of past ages is sufficient to show us the wisdom of such a course; and considering the constant tendency of the mind to rest on an assumption, and when it answers every present purpose, to forget that it is an assumption, we ought to remember that it, in such cases, becomes a prejudice, and inevitably interferes, more or less, with a clear-sighted judgment. I cannot doubt but that he who, as a wise philosopher, has most power of penetrating the secrets of nature, and guessing by hypothesis . . . will also be most careful for his own safe progress and that of others, to distinguish that knowledge which consists of assumption, by which I mean theory and hypothesis, from that which is the knowledge of facts and laws; never raising the former to the dignity or authority of the latter, nor confusing the latter more than is inevitable with the former."

MICHAEL FARADAY, *Phil. Mag.*, vol. 24, p. 136, 1844.

CHAPTER V

THE EYE-WITNESS'S OBSERVATION

Scientific studies of man's external environment start from observation. The basic sense data of science are got by seeing, hearing, handling, tasting, and smelling. Since the present study of research technique uses as basic the idea of agreement between different observers instead of the idea of absolute truth, it is especially necessary to subject human observation to a highly critical examination. Attention is therefore concentrated in this and the following chapter on finding when, if ever, agreement between human observers is reached. An attempt is made to determine what are the essential and what the desirable conditions of stimulus in order that sense organs may yield scientific observations.

Consideration of the following example will serve to illustrate one essential feature of all observations. Suppose one is writing or working in a laboratory and perhaps begins to feel tired. If the task need not be completed then it is quite common practice to decide to stop working when some clock strikes the hour or chimes. If, however, one is especially interested in the work, it is common experience to find that one has continued working longer than had been decided simply because the clock was not heard. In such circumstances the clock really has struck, other people have heard it strike, and there is no possibility that the sound waves did not impinge upon our own ears with sufficient intensity to be heard by us. An appropriate stimulus of suitable intensity has acted upon a healthy, normal sense organ, but has not yielded an observation. Words are available to describe this phenomenon. It may be said that *attention* was absent. Although the phenomenon is common, well known, and named, it is not understood. The essential differences between the condition of a stimulated sense-organ yielding or not yielding an observation are not known. Attention is, then, one essential requirement

of observation. But whether attention precedes, determines, or merely accompanies observation, is not known.

THE EYE-WITNESS

In man the eye yields far more data consciously used in scientific research than does any other single sense-organ. Its use is therefore worthy of special attention. What happens when men with good eyesight look at the same object? If each man looks at it in turn from precisely the same view-point does each man *see* precisely the same thing? Does each get the same mental impression or does each make the same report upon it? If the eye be first regarded as an optical instrument it can be treated as similar to a photographic camera. The pupil of the eye is part of a lens, the curvature of whose surfaces can be varied by muscular effort of a part of the eye. When we look at an object this lens is, in normal vision, unconsciously adjusted until it focuses an optical image upside down upon the retina at the back of the eye. The way in which the retina behaves as a surface sensitive to incident light and the way in which a message is sent to the brain are examined by the physiologist and the psychologist, and at present there is no evidence of any considerable difference in the normal vision of men with good eyesight. Let us examine therefore the reports of a number of men who have been looking at the same thing.

The first example is taken from an incident which happened at a Congress of Psychology at Göttingen.* Not far from the hall in which the meetings were held, a public fête with masked ball was taking place. During one of the meetings the door of the hall was suddenly opened and in rushed a clown chased by a negro carrying a revolver. After a scuffle in the middle of the room the clown fell to the ground, the negro leapt upon him, fired the revolver, and then both rushed out of the hall. The whole interruption lasted about twenty seconds.

As soon as it was over the President, explaining that there

* A. von Gennep, *La formation des legendes*, pp. 158–9.

was sure to be a judicial inquiry, asked those present to write there and then a report of what they had just seen happen. Forty reports were sent in, all of them written at once whilst the incident was fresh in the mind. Although the observers did not at the time know it the whole incident had been previously arranged, carefully rehearsed, and photographed. When the reports were examined it was found that only one of them had less than 20 per cent of mistakes about the principal facts, fourteen had 20 per cent to 40 per cent of mistakes, twelve from 40 per cent to 50 per cent, whilst thirteen had more than 50 per cent. Not only were mistakes made, but purely fictitious details were introduced so that in twenty-four accounts 10 per cent of the details were pure inventions. Six accounts contained fewer inventions than this, but ten included even more pure fiction. It will be noted that the whole incident was brief, lasting only twenty seconds, the details were so striking as to arrest the attention of almost any spectator, and these details were immediately written down by men accustomed to scientific observation. No observer was obliged to write a report, so that those who did send them in were not trying to evade being called as legal witnesses. None of the observers had any direct interest whatever in the result of any inquiry into the affair so none of the usual motives of false witnesses were present. Yet in spite of these favourable conditions only six of the forty reports were admissible as approximately correct accounts of the facts. It is true that all the observers had not the same viewpoint. Some of the details must have been hidden from some of the observers and it would therefore be unreasonable to expect all the reports to contain all the main details. What is especially to be noted is the quite unconscious substitution by trained observers of pure fiction for fact.

Many similar experiments have been carried out by different workers. Wolters* gives an example carried out during a lecture given to seventy undergraduates, the subject of the lecture itself being actually Observation. Two senior students made a sudden and violent entrance into

* A. W. P. Wolters, *The Evidence of Our Senses*, chap. iv, p. 49. 1933.

the room, engaged in unbecoming and noisy behaviour for a few moments and then quietly left. All members of the class were asked to write down there and then a detailed account of what had occurred. When they had finished their original reports they were asked to revise them, adding, if possible, answers to certain questions if the answers were not already in the original report. An accurate and full report would have contained ten essential points of detail. The average number correctly reported was 3·5, and on the average there was one completely false addition in each report. The nature of the fictitious details was very varied, including details which were physically impossible in the particular room and the seeing of an occurrence twice which had occurred once only. Only one observer out of seventy reported the noisiest episode. Later on the collected reports were examined by other individuals who were asked to write an account of the disturbance synthesized from the reports. They all found this impossible because the records were too widely and wildly inconsistent. As in the example previously quoted the incident had been made sufficiently striking and sudden to grip the attention of the observers, who were reported to be intelligent. Again there was no reasonable motive for deliberate falsehood. Rather one would have expected each observer to be trying hard to give a correct account of the incident in order to prove himself a good observer. In both these instances it may be objected that the witnesses were not engaged upon observation of the kind called for by the incidents to be reported.

Two days before writing these words, I had a direct experience of the difficulty of correctly reporting a short incident which happened in front of my eyes. I was driving a motor-car in a town well known to me and had been discussing with my passenger the unreliability of the evidence of eye-witnesses. At a crossing which was very familiar to both of us we noted that contrary to our usual experience the crossing was quite deserted, no other traffic being visible. The car was moving very slowly and we were both at the time looking ahead and not talking. Suddenly we saw what seemed to be a collision between a boy walking on the road

and a man riding a bicycle. The incident seemed suddenly to appear in the centre of the field of vision where there was at the time apparently no other object of interest. The incident was sufficiently far from the motor-car to be quite unalarming and yet sufficiently near to be very clearly seen. Yet when we had passed the crossing we each found that we could give no account of what happened. All my passenger noted was that the cyclist nearly collided with a motor which later appeared on the right-hand side of the road. I could only be sure that I noted the boy getting up from the road and stepping onto the pavement, whilst the cyclist was still making a very wobbling crossing from the left- to the right-hand side of the road before he actually got off the cycle.

There were no reasonable grounds for supposing other than that a correct optical image was formed on my retina. The details were not too quick to be followed by eye or at least they were not quick enough to give a blurred image on the retina. It is doubtful if I could justly be said to be paying no attention to the incident for my attention was at the time directed to that part of the road and yet I found that I was quite unable to say what had happened there. Moreover, I had no motive whatever for suppressing any knowledge I may have had of the incident, for the accident was so slight that it was not even necessary for me to stop the car.

Here then are three definite examples of human failure to see what takes place before our very eyes. In all these examples there is no justification for supposing any imperfection, abnormality, or failure in the sense organs themselves. Good eyesight and good hearing were in use. Moreover, these examples are typical of human observation in everyday life. Why is the eye-witness so unreliable?

OBSERVATION AND ACTION

Clarification of thought is often produced by inquiring into purpose. On thinking of motive and function it can be realized that the prime function of our sense organs is not

to yield the data of scientific research, but to enable us to satisfy our primary needs. Primary must be defined. Under primitive conditions and even in civilized conditions most men in most circumstances behave in such a way as to preserve their lives. Moreover, man cannot do scientific research as we understand it unless he keeps alive. To preserve the life of individual man and of the race, certain requirements of food, warmth, sex, and hygiene are essential. If these needs are called the prime needs of man, it is evident that the sense organs are best adapted to help him to satisfy these needs. Man's needs in scientific research are very far removed from his prime needs. The whole attitude essential in scientific research implies that the scientist is free to disregard his prime needs. On this view then the data of scientific research are obtained through organs which are not especially well adapted to supply those data.

Keeping in mind this point of view of the function of the sense organs let us now re-examine the previously quoted examples of false eye-witness. It should be remembered that the tendency in most human beings is to behave in such a way that the prime needs, as just defined, are first satisfied. In the first example, therefore, where a man carrying a revolver rushed into the room the tendency to behaviour of the men in the room would be first to make sure that their own lives would not be endangered. The first idea coming into the mind would be, 'Shall I dodge behind some solid object to escape damage from a possible shot?' or 'Could I seize the revolver to prevent any shot being fired?' Only when a course of immediate action related to these problems had been decided upon, could any attention be spared for observation free from prime motive. Moreover, during the whole of the time whilst there was any immediate danger from a possible revolver shot a part of the attention would be directed to this danger. In general all the retinal images and their consequences in the brain would first be sorted out in terms of whether or not they endangered the life of the observer. Later, ideas of interfering, of helping one of the men would be considered. Consideration of this

example shows the advantage of relating observation to action rather than to thought or influence.

In the second example, where a university lecture on Observation was interrupted, there would be less fear involved and more reason to expect more accurate reports. In the third example dealing with observation from a motor-car the retinal images would be sorted out in terms of what to do with the controls of the motor-car. The driver of a car has continually to observe the road in front of him, but the process consists chiefly in deciding from the nature of the retinal images or from the sounds incident on the ear what to do next with his hands and feet. As I consider this example, I remember thinking at the time I was glad that I saw the boy getting up unaided from the ground, as this implied that he was not seriously injured, and that I need not interrupt my meditations on the subject of observation in order to take an injured boy to a hospital. Notice how from the numerous retinal impressions I remembered one which enabled me to decide what to do next, namely, to drive on. Of the other retinal images I had, only a few seconds later, no knowledge whatever. It is true that a few seconds later I had started to think upon the incident. I found myself thinking that a cyclist had run into a boy who had stepped onto the road. I did not 'see' these things, I only inferred them from the actually observed behaviour of the boy and the cyclist. Much of the law court evidence of eye-witnesses relating to such incidents as motor-car accidents is, when not deliberately false, inference from observation of a few details only.

The results so far reached from all these considerations are that reliable observations from suitable stimuli acting upon sense organs can be obtained only if the observer is paying attention to the thing observed. It is general experience that the more suddenly the phenomenon happens and the more unexpected it is, the less likely are reliable observations upon it to be made. Accurate observations can be made upon transient phenomena, but less easily than upon slower phenomena. Even with a slow phenomenon or with stationary objects much detail cannot be observed

in a little time. The idea that when a highly skilled adult observer glances at an object he takes in every detail of the object seems fallacious.* Not a single detail escapes the glance of the lynx-eyed detective of fiction, but his like has never been found alive. It seems then that if the eye is used to observe it may be necessary not only to look *at* the detail to be observed, but also to look *for* it. We do not always find with our sense organs unless we also seek.

When as a physicist I go into a physiology laboratory and look through a microscope at a stained tissue section, I see nothing but a meaningless array of coloured patches and lines. Where I see only a grey ring with a spot in the centre the physiologist sees a section of a nerve fibre. When I show an X-ray crystal photograph to a physicist who has not researched in that subject, as like as not he first comments on the large black patch in the centre of the photograph, a patch caused by that part of the X-ray beam which has been unaffected by the crystal and is therefore of no interest to the X-ray crystallographer. He notes also the black patches where light has accidentally reached the film through pin-holes in the black paper screen, but the X-ray crystallographer is hardly conscious of noticing these parts of the film. When I am shewn a photographic plate of optical spectra I notice the calibration spectrum just as much as the other spectra which the spectroscopist expects me to be looking at. In all informal intercourse in laboratories devoted to different subjects the capacities of our fellow workers for first seeing the 'wrong' feature of any photograph or detail they may be shown, and in turn our own capacity for doing the same thing in their laboratories can frequently be noted. The demonstrator of experiments at a university exhibition of popular science has still more examples of this evidence that a part of observation consists in both looking at and looking for the object.

Suppose now that our very limited power of taking in much detail at a glance be accepted as a fact. What happens

* This statement may not fit some of the facts of the phenomena of eidetic imagery. See E. R. Jaensch, *Eidetic Imagery*, London, 1930, and also Chapter VIII.

when the skilled observer is allowed to observe some very simple stationary object for as long as he pleases? These conditions seem to be favourable for reliable observation. He may look at a metre-rule lying on a bench on the far side of the laboratory. He walks up to the rule and picks it up still looking at it. A metre-rule suggests the subject of size or dimensions. What will be the views of this observer on the size of the metre-rule? No matter from where he observes the rule the lens of his eye focuses an inverted image upon the retina. But the distance from the optical centre of the lens to the retina in his eye is constant, so that, applying the results of simple experiments in the optics of lenses, it can be taken that the size of the image on the retina will vary with the distance of the rule from his eye. The further the rule is away from the eye the smaller the size of the image upon the retina. In spite of these acknowledged facts the observer does not get the impression that the size of the metre-rule or of any object varies with his distance from it. Clearly then the examination of even a simple object under favourable conditions is not a simple process. Perhaps we unconsciously add something derived from previous experience.

Let us observe a simpler object than the metre-rule. With pen or pencil make three dots in any arbitrarily chosen position not too far apart, upon a sheet of paper. On looking at these what does one see? Not, I think, just three dots, but three dots at the corners of a triangle. It is beside the point to inquire here into the question of why the three dots are seen as part of a triangle and not for example as part of a circle on which they might equally well lie. It is enough for the present purpose to realize that we cannot, in observing the three dots, see easily just them alone without seeing also their relation to a triangle or to some figure. Even when this is pointed out it is still difficult to see the three dots without their triangularity. The phenomenon is not restricted to three dots. Four dots may be seen as part of a square, oblong, or other four-sided geometrical figure. As the number of dots is increased the tendency to see more of the pattern than of the actual dots is

more pronounced even though the dots are big enough to be clearly visible individually. Expositions of the theory of

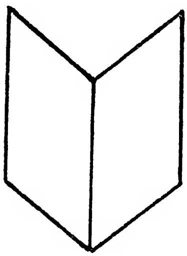

FIG. 1

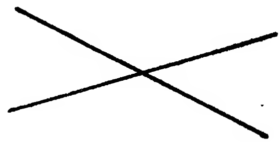

FIG. 2

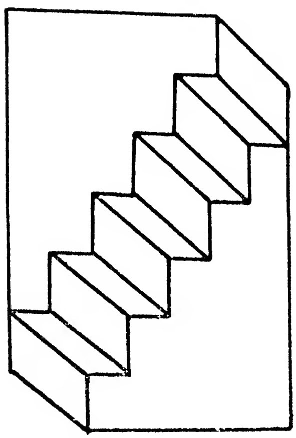

FIG 3

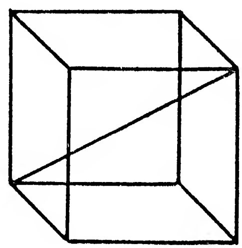

FIG 4

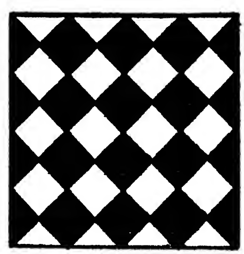

FIG. 5

FIGS. 1–5.—Diagrams to illustrate a fluctuating element in steady observation. The appearance of each figure alternates between two distinct forms each of which may remain unchanged for several seconds. (See text.)

crystal lattice structure are often illustrated with a figure consisting of a large number of dots arranged at the corners of squares of uniform size. As the exposition dealing with

the so-called crystal planes is read, first one kind of row or column of dots and then another is seen, although the whole figure remains unchanged.

In steady observation of some of the equivocal diagrams found in the literature of optical illusions a fluctuating element seems to be present. This can be tested by looking at Fig. 1, preferably with one eye shut. The figure seems so simple that it is difficult to know what to look for. Yet one seems to be conscious of effort, and the diagram changes from a folded sheet of note-paper with the fold nearest, to one with the fold farthest away. Fig. 2 can be seen as two straight lines intersecting at an acute angle or as two straight lines intersecting at right-angles but seen in perspective. Fig. 3 shows similar changes in the appearance of the stairs if observation is continued for some time. In Fig. 4 first one of the squares and then the other seems to form the nearer face of a cube seen in perspective. Fig. 5 illustrates how, in continuous observation, sometimes one part and sometimes another is seen as the most important feature. The diagram seems to be sometimes a white pattern on a black background, and at other times a black pattern on a white background. Visual observation of these five figures does not yield erroneous information. They are offered here as evidence of the complexity of the continued observation of even simple figures. The diagrams are deliberately drawn to suggest familiar objects, but to be sufficiently indefinite to leave at least two simple interpretations. Leisurely examination of the figures seems to reveal some kind of fluctuating element as one characteristic of pure observation. The same feature can be noted in the examination of patterned wall coverings or fabrics whether they be of old design using the shapes of common objects, or of present-day design in which no use is made of the shapes of actual objects. First one part and then another of the pattern seems to attract the observer's attention no matter how familiar he may be with the pattern.

INFERENTIAL OBSERVATION

Some examples of visual observation which in the phraseology of inference may be said to yield results definitely erroneous will now be considered.

> ". . . in the night, imagining some fear,
> How easy is a bush supposed a bear!"

This type of illusion illustrates both the ever present attention to self-preservation and the association of the retinal image and action. In Fig. 6 the dotted space at the left is as long as the open space on the right although it appears longer. The two central horizontal lines in Fig. 7 are parallel straight lines although they appear curved. In Fig. 8 the illusion of Fig. 6 is reversed by putting one instead of many dots on the left-hand side of the central dot. In Fig. 9 the two central circles are the same size although the right-hand one appears larger. Which of the two lines on the right-hand side of Fig. 10 is the continuation of the sloping line on the left-hand side? The use of a straight-edge shows the error here of one's unaided observation. Fig. 11 is the familiar illusion in which the two vertical lines of equal length appear to be of unequal length.

The eye is not the only sense organ which may yield false or misleading information. Most text-books of heat in introducing the subject of temperature describe a simple experiment showing the unreliability of the normal sensation of warmth and cold. Simultaneously one hand is held for two or three minutes immersed in hot water and one in cold. Both hands are then shaken and plunged into tepid water.

To the hand which has been in the hot water the tepid water feels cold, to the other hand it feels warm. Here the two hands of the same observer yield at the same time a contradictory result about the warmth of the same object.

The unaided ear cannot be trusted always to give reliable information. One tone may be so masked by the presence of others as to be apparently absent from the sound heard. Sounds which in some circumstances would seem loud, in

other circumstances would seem of only medium loudness. The difficulty of judging correctly the position of a source of sound is well known. Recent research* has revealed a

FIG. 6

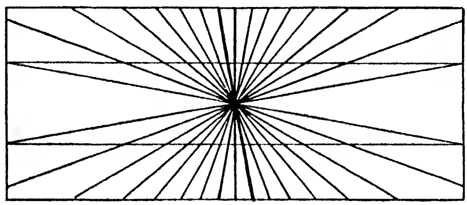

FIG 7

FIG. 8

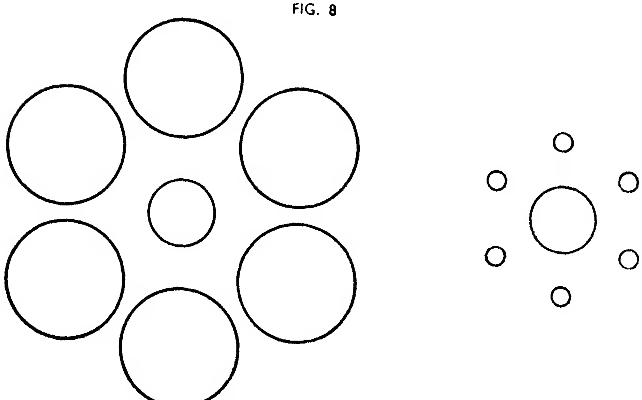

FIG. 9

FIGS. 6–9.—Diagrams to illustrate errors of inferential observation.

phenomenon which cannot easily be fitted into the idea of judgment of pitch of a complex sound, as a process of

* H. Fletcher, (i) *Speech and Hearing*, p. 246. New York, 1929; (ii) "Loudness, Pitch and the Timbre of Musical Tones and Their Relation to the Intensity, the Fequency and the Overtone Structure." *Journal Acoustical Society of America*, vol. vi, pp. 59–69. October 1934.

analysis into Fourier components. A musical note can normally be analysed into several pure tones of different frequency or pitch. The pitch of the lowest of these pure tones, whose frequencies are related by simple numbers, is usually judged to be that of the musical note. However, on hearing a musical note from which the lowest tone has been removed by the use of an electrical filter circuit, the pitch of the note is still judged to be that of the absent lowest tone. In all these examples of what are usually called sense illusions, it is found that the observer's inferen-

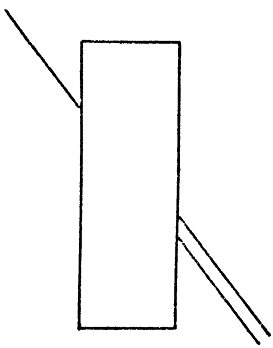

FIG. 10

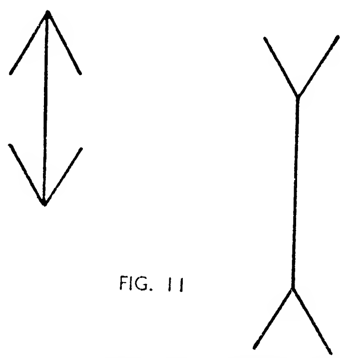

FIG. 11

FIGS. 10–11.—Diagrams to illustrate errors of inferential observation.

tial observations do not agree with his 'coincidence observations' of the same objects and events.

A pause may be made here to consider what results have so far been obtained in the critical examination of observation. Observation gives the facts upon which all scientific knowledge is based. In the first chapter facts were described as impersonal observations and impersonal was used in the sense of being essentially independent of any one individual, not as independent of human beings altogether. Methods of observing are therefore wanted which shall give the same results with all or at least with the majority of observers. Our examination has shown that the eye-witness of everyday life is from this point of view wholly unreliable. What is observed depends upon who is looking. To get some agree-

ment between observers they must be paying attention; their lives must not be consciously in danger, their prime necessities of life must preferably be satisfied and they must not be taken by surprise. If they are observing a transient phenomenon the phenomenon must be repeated many times and preferably they must not only look *at*, but must look *for* each detail. In inferential observation of even a simple stationary object its appearance may seem to change if it is viewed for a time (Figs. 1–5). Inferential observation of other simple objects may give erroneous results and the erroneous results are not permanently absent from unaided observation. The deceptive appearance of some optical illusions remains even with knowledge of the more detailed evidence. No method of observation has therefore been found which can be guaranteed to give trustworthy results usable as basic in scientific research. A description of a reliable method is, however, offered in the following chapter.

CHAPTER VI

SCIENTIFIC OBSERVATION

"'What is Truth?' said jesting Pilate."

It is well known that in scientific research much use is made of instruments and apparatus to aid the senses. Direct observation is not always used. The sensitive photographic surface, the photo-electric cell, and the thermo-electric couple are often substituted for direct vision. Sound vibrations are converted into electrical oscillations and are analysed and measured in that form by instruments having a range far beyond that of the human ear. The balance is substituted for the kinesthetic muscular sensations in the estimation of relative weight. Thermometers and pyrometers are substituted for the sense of touch in estimating warmth and coldness. Where a phenomenon occurs rarely and then only for a short time as in solar eclipses, recording instruments are substituted almost entirely for direct visual observation. With the added complication of unexpectedness, as in earthquakes, the seismograph continually waits ever ready to make records upon which observations can be made later. In direct observation the eye is often aided by the metre-rule, the lens, microscope, telescope, and other optical instruments.

COINCIDENCE OBSERVATION

Let us examine a little more carefully what kind of human judgments have to be made when direct observation is aided by various instruments. Attention has already been directed to the unreliability of judgments of the warmth or coldness of objects touched. The sensations got through the two hands of the same observer may lead to contradictory results. If, however, a simple thermometer such as a clinical thermometer is used, the actual observation is not a judgment of warmth. The act of taking a patient's temperature

consists in seeing against which mark on the glass the mercury comes to rest. The observer has to judge with which mark the end of the mercury column coincides or if it lies between two marks he may judge to which it is nearer. This is a typical judgment of coincidence. In weighing with the chemical balance the observer has to alter the weights in one of the scale pans until a pointer, which moves over a scale whilst the balance is swinging, comes to rest opposite one mark on the scale. In many of the balances now used in shops a scale marked on a cylinder moves behind a line marked on a glass window, and the same judgment of the coincidence of two marks has to be made. A similar kind of judgment is necessary to the fine tuning of many radio reception sets. It is in this kind of observation that something like universal agreement can be reached. It is not here necessary to consider the distance apart of the various marks of a scale or the numbers placed against the various marks. It is sufficient to note that this kind of judgment is one which can be made readily by all normal human beings. No elaborate training is necessary. The shop assistant and the analytical chemist in making weighings both have to make the same simple judgments of coincidence.

It is, then, from this kind of observation that impersonal data can be got to form the basis of scientific knowledge. The method does not eliminate the human observer, but he is given a task which from experience is found to be well within the powers of the majority of men upon the majority of occasions. The making of judgments of coincidence seems to include all the judgments in which universal agreement between men is ever found. If men are asked to judge values of truth, beauty, goodness, importance, merit, or in fact values of any kind, not even an approximation to universal agreement is in practice found, and we enter a world of constant conflict. If the same men are asked to judge coincidences, we get the nearest to universal agreement that is ever found in working with biological material. Any man who can tell the time of day by the clock can judge coincidences and in so doing he is making coincidence observations whether or not he is a scientist.

In the so-called exact sciences this type of observation is always used and is often combined with more complex processes, including counting. The results are then called measurements. The tendency in all scientific work is to try to devise methods of observing the phenomenon studied so that the final observations are judgments of coincidence of two things. Familiar examples are in the use of the rule in measuring lengths, the clock, the balance, the measuring cylinder, flask, burette or pipette, the galvanometer, the spectrometer, and the thermometer. One type of exposure meter used in photography depends on a matching process. In the use of all these instruments the judgment of coincidence is made by eye.

It is not necessary here to consider whether or no a judgment of coincidence is the simplest which can be made by man or whether it can be analysed into or related to, for example, judgment of one thing being between two others, or judgment of one thing being nearer to another than to a third thing. In the laboratory there is always a stage before the final observation when two lines or marks do not coincide or when two luminous areas are not matched. Adjustments are made to the apparatus and then the judgment of coincidence is made by the observer. In the 'null' method of using some scientific instruments the observer has always to make three judgments for each 'reading.' These three are, that when certain alterations are made to the apparatus a pointer or an optical image, thrown onto a scale, in turn is on one or the other side of a reference line or coincides with the line.

Similar judgments of coincidence can be made by the use of other senses. Whether two sounds occur together or whether they can be distinguished as two separate sounds can be judged by ear. It is interesting to note that in his experiments on falling bodies Galileo used this method of coincidences of sounds to tell whether two bodies reached the ground together, by allowing them to fall onto an iron plate. When our eyes are fully occupied in the laboratory so that a stop-watch cannot be observed, the power of judging coincidences with other senses can be used by fitting

muscular actions into the time pattern set by a ticking metronome. By this means visual observations can be taken at times marked by the sounds heard. Outside the laboratory in dancing to music bodily movements are continually fitted into coincidence with time patterns. With the sense of touch, whether or no two coins fit together and are of the same size can be judged, and a blind man can similarly tell the time by judging the fit of the hands of a watch with raised marks on the watch face. Although visual judgment of coincidence is more widely used in research than is any other, any such observation is in this book referred to as a judgment of coincidence irrespective of whether sight or hearing or any other of the senses are used.

THREE CRITICISMS OF COINCIDENCE OBSERVATION

Three objections are sometimes raised to the claim of the coincidence observation to be regarded as representing a human judgment or observation in which there is a very near approach to universal agreement between men. It will suffice to discuss them as illustrated in visual judgments of coincidence.

Firstly, if, for example, the time indicated by the seconds hand of a watch whose mechanism is not working is read, and if then the end of the seconds hand be examined with a magnifying lens it may be found that the pointer does not seem to coincide so well with the mark of the seconds circle. To overcome this difficulty, in looking at the watch with the unaided eye the seconds hand must be regarded as the pointer, but when a lens is used some part only of the hand must be chosen to act as pointer and one edge only of the scale mark must be used in making the judgment of coincidence between them. In general, a judgment of coincidence is always to be regarded as establishing coincidence up to the degree of resolution or of detail available to the observer. In the laboratory one usually uses only the edge of a pointer and the edge of a line, as it is difficult to make fine pointers sufficiently robust to avoid damage in use and sufficiently visible to avoid eye strain.

The second objection to the reliability claim of the coincidence observation made by eye is due to the phenomenon of *parallax*. If a watch face be slowly turned away from the observer's eye then a hand of the watch may appear to coincide with different marks of the watch face according to the point of view. This apparent change of position of an object due to the change of the point of view is known as parallax, and is avoided in the making of coincidence observations by using an optical image projected onto the scale, or alternatively by placing a mirror underneath the pointer and near the scale. In this latter case the observer first moves his point of view until the pointer coincides with its optical image in the mirror. He then makes the judgment of coincidence with a mark on the scale.

In considering the third objection to the universality of coincidence observation the full significance of a part of the first chapter will become apparent. It was stated that facts would be taken as forming the basis of science. Facts have now been technically defined in such a way that any person knows what test to apply. The test, that of coincidence, is based upon a property found in practice to be common to all men. The basis of science is therefore accessible knowledge.

Consider now, as a third objection to coincidence observation, such a statement as: "If an observer in making a coincidence observation has taken care to avoid the parallax error and has specified carefully the parts of the two objects between which he has observed coincidence, how does he know that the two objects really coincide? Suppose he has used a microscope with oil immersion to get the highest visible magnification possible, or suppose that he has even used the ultra-microscope. If an X-ray microscope could be devised and used it might be found then that the two objects did not really coincide. Furthermore, in testing the universal agreement of different observers, how is lying to be dealt with? A man may look at a watch which indicates to other men, six o'clock, but he may say that it indicates any time." The first part of this objection depends upon the word 'really.' This, in its use here is a philosophical

term and is therefore forbidden to the patternist. The second part of the objection depends upon the word 'lying.'

Since the patternist treats facts (as technically defined) as basic, and does not use the idea of truth (in any philosophical sense of the word) he would at once reply that he never knows whether two objects 'really' coincide. He has taken facts (coincidence observations) as basic, and must therefore define 'really' or 'lying' in terms of coincidence judgments made by human observers. It would be logically inconsistent for him to attempt to use both human judgments of coincidence and also the idea of truth as basic. The choice of either, as basic, can be logically sound. But the attempt to use both as basic is an attempt to run with the hare and hunt with the hounds. The results of judgments of coincidence made in such institutions as the National Physical Laboratory near London, where standards of length are compared, have to be so stated that equally skilled workers in institutions in Washington, Paris, Berlin, Leningrad, and elsewhere reach the same results when examining the same objects. The patternist uses such results because they are found in practice to be constant. He does not ask how to set the cross wires in the different microscopes so that they 'really' coincide with the reference marks on the standards, but asks, if he is a physicist, for technical details of what he must do. That is to say, he asks for definitions to be given in terms of action, instead of in the technical terms of philosophy.

Similarly 'lying' must be defined in terms of observable action. As an illustration of what might be done to test if a man could judge coincidences he might be locked without food in a room, the door of which was fastened with locks of the type used on some safes. The inside of the door could be fitted with a number of dials which had to be turned to bring certain marks into coincidence. By the side of each dial could be placed a diagram showing the state of coincidence necessary to enable the door to be opened from the inside. If the man escaped, say, twelve times in one hour, by setting the dials to the indicated positions it could be said that his judgments of coincidences were the same

as those of other men. The technical details may need elaboration, but the idea of defining in terms of observable actions, instead of unobservable truth, is sufficiently well illustrated. The physicist may find interest in defining in terms of human action what he means by saying " . . . but the electron may not really be in that position."

In making the claim that the judgment of coincidence covers all judgments in which men at present reach universal agreement, there is no suggestion that the powers of different individuals are identical in making these judgments. For example, one man may be quicker in making a number of laboratory coincidence observations, another may detect a minute 'error' or lack of fit where coincidence was seen by another observer. These small differences between the coincidence judgments of different individuals have been extensively studied by physiologists and experimental psychologists, and the results can be found in the literature dealing with sensory acuity and discrimination.

In man's power of judging fit and lack of fit, a human quality of the greatest service in the study of research has been found. It is the means by which the basic data of science are obtained and tested. The terms judgment or observation of coincidence will be used as equivalent and fact will be defined as that which can with the current technique be observed by judgment of coincidences.

Coincidence observations made with apparatus fitted with scale and pointer such as a watch face or a thermometer, and also observations which could in the current state of knowledge be so made, are all accepted as facts. This can be illustrated by a few examples: If a chemist adds one liquid to another and observes the formation of a precipitate or an evolution of gas or a change of colour occurring in the mixture, his observations would be here called coincidence observations and would be regarded as suitable data for use in scientific research. The precipitate or the bubbles could be observed as coincidences against the marks of a squared eyepiece of a microscope, and a change of colour could be observed by the method of coincidence by the use of the spectrometer or the photo-electric cell and

galvanometer. No account is taken of the difference that the observer has not actually used these instruments. Similarly the observation of the form, shape, or colour of stained tissue sections or botanical specimens or crystals under the microscope would be called coincidence observations. A man's speech or writing or muscular actions or flushing would all be regarded as observable by judgment of coincidences even though instruments may not have been used in making the observations. The observation of the patch of light on the fluorescent screen of a cathode ray oscillograph in use would be called coincidence observation. The observation of the flow of saliva when food is placed before a hungry dog would be called coincidence observation.

From these examples it will be evident that measurement is not regarded as an essential part of making observation by the coincidence method, although to make measurements judgment of coincidence must always be used. Throughout this study judgment of coincidence is used rather as a test to be applied in case of doubt' It enables us to know how to test our basic material before giving serious thought to its interpretation.

SUMMARY OF THE TECHNICAL DEFINITION OF A *FACT*

An examination of human observation shows that agreement between different observers is readily reached without the use of threats or torture if the different observers are set to judge coincidences. These elementary human judgments give the nearest to universal agreement that is ever reached, and are called either coincidence observations or facts. The following notes on coincidence observation serve to define the sense in which the term is used throughout this book.

(1) A common example of the use of coincidence observation in everyday life is in telling the time indicated by a clock, when the observer judges whether or no a hand of the clock coincides with a mark on the clock face.

(2) No measurement can be made without the use of coincidence observation, but coincidence observation is *not* necessarily a measurement. Hence in the example of Note (1),

the subsequent reading of the number against the mark on the clock face is not here regarded as part of the elementary judgment involved in using coincidence observation. The clear distinction between making an elementary judgment of coincidence and the more complex processes, including combining this with counting to give measurement, is essential for an understanding of parts of this book.

(3) An object is said to be observable by the coincidence method *only* when it can serve as one of the two things between which coincidence is judged. That is, the thing to which the name of the object is attached must directly give sense data; an electron, for example, cannot by definition be observed by the coincidence method.

(4) Coincidence observation is used as a means of separating the sense data usable as basic in science from other sense data and from inference. The qualification of Note (2) admits such sense data as the sense judgments made in adjusting an optical photometer or any other 'matching' or 'null' method used with the aid of any of the sense organs. These are not measurements.

(5) Provided that the rule of Note (3) is followed there is no insistence that the research worker must have used instruments fitted with scales and pointers, when observing his basic facts. Throughout the book the term coincidence observation is used for any observation which, with the current laboratory or field technique, could be observed by judgments of coincidence, no matter whether it has actually been so observed or not. The method is actually applied wherever convenient and is invariably applied in cases of doubt or controversy. In the latter case it serves as the final arbiter at the time it is applied. A dispute about what can be observed in an experiment "is to be decided not by discourse, but by new trial of the experiment."*

These statements may be roughly summarized as: Science is based upon human judgments of coincidence. This form differs profoundly from the statement: Science is based upon measurement.

* Newton in a letter dated November 13, 1675.

INTERNAL OR PERSONAL OBSERVATION

It may seem on first thoughts irrelevant to consider here another kind of observation which cannot be made by judgment of coincidences and which is not used as a tool in scientific observation. But again it must be remembered that research work is done by human beings, not by machines, and consciousness is a property of man. Stimulus of sense organs may not give an observation if the observer is not paying attention; that is, if he is not aware or conscious of the stimulus. In making observations either with or without the aid of instruments are we conscious of nothing more than the things which can be examined by judgment of coincidences? In practice it is found that we are conscious of other things and that in the present stage of knowledge these other things cannot be examined by coincidence methods. As these other things seem to exert a powerful influence upon us, and seem very close and 'real' to us, they may possibly affect what is done with coincidence observations. It will therefore be regarded as relevant to consider them here. For convenience of reference all such observations which can be studied only by the method of introspection, will be called personal or internal observations to distinguish them from coincidence observations which might be called impersonal or external observations. One or two examples will suffice.

Suppose a man observes a scarlet anemone. He may be conscious of its form and colour and texture and all of these can be studied as coincidence observations. If attention be confined to, say, one of these, the colour, a petal may be illuminated by white light and examined by the spectroscope or with the aid of the photo-electric cell and other instruments, but the sum total of all these coincidence observations of the colour of the petal does not represent all of which the man is conscious relating to the petal before him. To take another example, if a man is suffering from toothache he is certainly conscious of something other than the currents passing along the tooth nerves. He is in fact not conscious of these currents as such. The nerve currents,

if they can be observed, are impersonal observations, and the pain is a personal or internal observation which only the man himself can observe. Anger and fear can be similarly examined. In the angry man the external observer may observe such things as a stiffening of the body, flushing, loud speech, temporary holding of the breath, increased blood pressure and pulse rate, and possibly an increase in the amount of blood sugar and in the secretion from the adrenal glands. Some of these changes could be observed by the angry man himself, but in addition he would be conscious of other things about his anger which could not be observed by coincidence methods. These other things would be the personal and internal observations, and whether they are called anger or whether the term is applied to the sum of both the types of observation is a matter of definition. For the present purpose it is enough to note that two different kinds of observation can be made upon an angry man and that one type can be made only by the man himself. Except in the study of introspection the internal type of observation is not consciously used in scientific research. Whether or no it is supposed to have any influence upon the actual carrying out of research depends upon what theory of human action is used. In the formal study of scientific method no account is consciously taken of internal observation, but it would appear that formal scientific method would only yield scientific research results if pure-reason machines were available to apply the method to nature. As it is, research is done by human beings, and they cannot avoid making these internal observations as well as the external or coincidence observations.

This brief discussion of internal observation serves as a caution against saying that in observation, nothing else matters but coincidence observations. To remain scientific, no such extravagant claim must be made. It may be said that men have so far found no other method of observation which in practice leads to such complete agreement between different observers. It is unscientific, however, to say that any object or phenomenon is *nothing but* the aspects of it

which can be studied by the coincidence method. When an observer looks at a bed of primroses in a sunlit wood in springtime he must quite frankly admit that some of the things of which he is conscious, and some of which he is not conscious, can in the present state of knowledge be studied by the coincidence method, but others cannot. It may be human, but it is certainly unscientific to refuse to make this admission.

ANALYSIS OF STATEMENTS OF OBSERVATION

The statements of results of observations obtained in scientific research are by no means always given as coincidence observations. In the original publication in scientific journals they can often be found in this form in measurements, but even there the observations are discussed and 'interpreted.' Later the interpretations are said to have been observed. The more familiar the interpretation becomes, the more are observations which fit into the interpretation referred to in terms of the interpretation alone. Gradually as more and more experimental results fitting the original interpretations are obtained, general terms are necessary for enumeration. There is then a danger that those who are not intimately familiar with what the research workers have done in the laboratory may be unable to separate observation from interpretation. Such difficulty has been found in quantum physics and in certain branches of psychology.

It will be found that wherever statements made about complex scientific observations lead to apparent anomalies these statements can be analysed into three parts. This analysis will often help in clarifying thought about the subsequent use to which the observations are put. The three parts are:

(1) Statements of the external or coincidence observations; that is, the judgments of coincidence made with the aid of one or more of the sense organs.
(2) Statements of the internal or personal observations; those things connected with the subject observed,

of which the individual observer alone is conscious and which cannot be observed as coincidences. (These data are used by some psychologists.)

(3) General terms used either to refer to collections of all the separate types of coincidence observations (1) so as to avoid the necessity of enumerating each in turn when the whole group is referred to, or alternatively general terms relating to hypotheses or theories into which the observations are fitted.

This way of analysing statements about what has been observed in scientific research can be illustrated by discussing in turn two typical questions, one taken from biology and one from physics. The questions are, firstly: "When a salmon is seen swimming up stream and leaping at a waterfall are we observing an example of purposeful activity?" and secondly: "Is the observation of an object such as a laboratory bench or a chair the same as the observation of an electron?"

Considering first the biological example it is evident that no internal observations can be made. The external observations which could with sufficient trouble be made as coincidences with the aid of cine-camera and other instruments, would consist in noting the water flowing in one direction and the salmon repeatedly leaping in the opposite direction, falling back, and finally reaching the part of the stream above the waterfall. With a sufficiently elaborate experimental technique, observation could be made of changes in the amount of lactic acid in the muscles, of minute currents passing along the nerve fibres and of many other physiological and biochemical changes. When a large number of these different coincidence observations had been made it might be found that similar results were obtained in many experiments on salmon leaping waterfalls. It would then be convenient to have some general term to refer to the whole group of coincidence observations. Pulse rate might be called activity or observation of type number one, or briefly A_1; change of lactic acid content in certain muscles, A_2; and so on. It may then be said that

$A_1 + A_2 + A_3 + \ldots A_n$ have been observed. If 'purposeful activity' is only another symbol for the sum of all these separate observations so that $\Sigma A_n =$ *purposeful activity* may be written, then it may be said that 'Purposeful activity' has been observed by the coincidence method. If, however, the term 'purposeful activity' is used to describe some hypothesis or theory to fit all the separate observations $A_1, A_2, \ldots A_n$, then purposeful activity has not been so observed.

In considering the water flowing over the waterfall a number of observations could be made which could be summarized by saying that the water showed purposeful activity or was striving to get to the sea or to sea level. For example, some of the water could be stored in a bottle and later when released at the top of the fall, could be observed to continue on its journey to the sea. Some of it might evaporate, become cloud and later fall as rain into the sea or on the land when it might find its way into some stream and ultimately reach the sea after many vicissitudes. If 'purposeful activity' summarized all the separate incidents between the water arriving at the top of the waterfall and reaching the sea then it could be said that the purposeful activity of the water had been observed. If, however, the term referred to a theory to explain the separate incidents then it would be unscientific to say, with the proposed definition, that the purposeful activity of the water had been observed. Incidentally it may be noted that such theories to explain the motion of inorganic matter were in favour at one time. General terms are often of the nature of hypotheses or theories rather than names or symbols for groups of coincidence observations. It is only in the latter case that they are referred to as having been observed.

The case already discussed of observation of an angry man would be similarly analysed, except that here, if the observer was the man himself, some internal or personal observations would also be possible. Anger would be said to be observed by the coincidence method if the word meant the sum total of all the separate observations of flushing, increased pulse rate, and so on. If it referred entirely to what

the angry man alone could observe then it would be said to be internally observed.

OBSERVATION OF AN ELECTRON

The second example to be analysed is relevant to the verbal interpretation of certain results obtained in theoretical physics. It may be asked 'Is the observation of an object such as a chair the same as the observation of an electron?' On the proposed method of analysis each of these two observations must be analysed into three parts. The external or coincidence observations which can be made upon a chair are many. If sight is used the coincidence of feet with marks upon the carpet or of its outline with other objects in the neighbourhood or of parts of it with marks on other objects such as a metre-rule can be observed. If touch and kinesthetic sensation are used many more observations can be made upon the chair by feeling it, sitting in it, or by knocking a part of the body against it. In the latter case further observations can be made by the sense of hearing. By the method of internal observation the individual can observe such things as beauty or ugliness in the form of the chair, or the comfort of relaxing in the chair when he is tired, or the memory of who has sat in the chair before. Each observer can make these observations only upon himself. As for the third section of the proposed method of analysis, the word chair seems to represent the sum of all these observations, external and internal. The reader who is inclined to think that the word chair refers rather to a hypothesis, additional to the idea of external objects, is asked to defer judgment until he has read Chapter VIII relating to the direct observation of pattern or wholes.

On applying this threefold analysis to observation of the electron it is evident that no observations of the first kind can be made. An electron cannot consciously be seen, touched, heard, tasted, or smelt. It cannot be observed by judgment of coincidences. When a cathode ray oscillograph is in use a stream of electrons is said to be hitting the screen but is not seen as such. Only a light green patch of light

on the screen is observed by the coincidence method. In a Wilson cloud chamber by the same method can be observed tiny drops of water lying along what is called the path of an electron moving at high speed. In all experiments in which one or more electrons are said to be involved, coincidence observations can be and are made, but not upon the electron itself. As for internal or personal observation of the electron, this also seems to be impossible, for observing the electron cannot be observed in the same way as an individual can observe his toothache (which no other individual can observe). It is evident that our third factor in the analysis of the observation of electrons is a hypothesis or theory into which many coincidence observations can be fitted. It cannot be a sum of a number of separate observations, for neither external nor internal observations have been made upon electrons. When this threefold method of analysis is applied to observation respectively of a chair and of an electron, it is seen that the essential differences are that whilst observations upon a chair can be made by the only method in which agreement is reached by human observers, that is, by the judgment of coincidence, such observations upon an electron have not so far been made. I do not regard as vital to the present discussion a judgment of whether the word chair refers to an hypothesis or a summary of separate observations.

As a representative statement of a rather different analysis the following passage is quoted. It is taken from a discussion between Prof. P. M. S. Blackett and Prof. H. Levy.* Although the discussion was not specifically upon observation, the quotation deals with the comparison of observation of a chair with that of an electron. Although a number of philosophical terms relating to 'reality' and continued 'existence' of an external object are introduced and are outside the scope of this book it was thought safer to quote the complete statement as representative of a philosophical method of analysis different from the threefold method in which the use of philosophical terms is forbidden. Prof. Blackett was asked: "What is an electron?" and he replied:

* *The Listener*, vol. xi, No. 284, p. 1049. 1934.

"Scientists invented the conception of atoms and electrons: they are not things directly experienced individually by the senses so of course they are conceptual inventions made by man. But they are real none the less. It is just the same with the ordinary objects of everyday life. The complete idea of a solid object such as a chair or a table is not given directly to the senses. One only perceives certain aspects at a time. One can't see all round it at once, and one can't know it is there when one is looking the other way. In fact, the continued existence of a three-dimensional chair is a scientific hypothesis of exactly the same nature as the hypothesis of the existence and properties of electrons. Electrons have different properties from chairs and tables, but they are no more fictitious; only they are rather more abstract in the sense that they are several steps further removed from the direct perception of the senses than are the ordinary objects of everyday."

THE "OBSERVABLE" OF QUANTUM MECHANICS

The discussion of observation of an electron leads to certain results of quantum mechanics which in some writings upon popular science have been misinterpreted and have been stated even to give evidence in favour of the rationality of 'belief' in human free-will. A brief discussion of two points, the 'observable' of Dirac's quantum mechanics and the electron microscope of Bohr-Heisenberg, will suffice to illustrate that the observation referred to in quantum mechanics is not always simply related to what human observers can see, but is rather a mathematical or mental concept. A very clear exposition of the quantum mechanics 'observable' of Dirac is given in his *Principles of Quantum Mechanics* (1930). Speaking generally he states quite clearly that "The object of quantum mechanics is to extend the domain of questions that can be answered and not to give more detailed answers than can be experimentally verified" (p. 7), or again "The only object of theoretical physics is to calculate results that can be compared with experiment, and it is quite unnecessary that any satisfactory description of the whole course

of the phenomena should be given" (p. 7). In order to find how much knowledge of observation by human beings is necessary to understand this form of quantum mechanics a crucial test can be applied.

Instead of trying to form continually a picture of what can be observed to happen in some problem under discussion, let us go to the other extreme by starting from the symbols used in the algebra, calling each by some word which has no rational meaning for us. One way of choosing such words is to reverse the letters of the words used by Dirac to name the symbol. Two types of symbol called by Dirac 'state' and 'observable' become respectively *etats* and *elbavresbo*. Now if this is done throughout the exposition it will be necessary to give to the word *elbavresbo* only such meaning as can be found from the postulates. For example, it may be read: "In quantum mechanics it is more convenient to deal with something that refers to one particular time instead of to all times, analogous to the value of a classical variable at a particular instant of time. We shall call such a quantity" an elbavresbo. "We can now say, in both classical and quantum mechanics, that any observation consists in measuring" an elbavresbo, "and the result of such an observation is a number" (p. 25). Thus the value of a Cartesian co-ordinate of an electron at a particular time t_1 would be an elbavresbo. It will be found that if the meaning of the exposition could be followed before the substitutions were made, that is to say that if the reader has received a certain training in mathematics, the work can still be followed after all the substitutions have been made. Now this implies that to understand Dirac's work we do not need to know anything about the use of, for example, a microscope.

When an experimental research worker sees the words 'an observable,' he automatically thinks of such an operation as setting the cross-wire of a microscope upon some mark on an object. For him the Cartesian co-ordinates of the centre, or of one edg of a tiny drop of oil seen under a microscope with squai eyepiece could be determined by judgment of coincidei. ces. On reading the statement

"The value of a Cartesian co-ordinate of an electron at a particular time t_1 would be an observable," one is apt to think that this refers to what a human observer could observe by the same or a similar method used with the oil drop. This is not what is meant in quantum mechanics, and any possible confusion with laboratory observation is avoided by using such a word as elbavresbo. With the substituted word the idea of observation by a research worker in a laboratory would not enter the mind, and an understanding of the mathematics would be helped by the avoidance of certain physical considerations with which the mathematics is not directly concerned. For the present purpose it is unnecessary to discuss what is meant by the quantum 'observable.' Although the idea is not simple it is more clearly explained by Dirac than are such terms as truth or reality or existence or value when used in any philosophical works I have so far found. It is sufficient for the present purpose to note that if in reading the exposition all laboratory ideas which the word observable conveys are given up, then the reasoning can still be followed by such aids as using the word elbavresbo instead of observable. This fact together with the fact that Dirac's postulated qualities of an *observable* are not concerned with properties of human observers, such as their power to judge coincidences, shows that the *observable* of quantum mechanics must be treated as a mathematical concept.

On this view a discussion of the quantum observable is irrelevant in a chapter dealing with human observation. It is included here solely because certain writers appear to have supposed that a quantum observable means something which a human observer can observe. The lack of distinction between a mathematical or other mental concept and something which can be observed by a human observer using the method of judgment of coincidences, is liable to cause as much confusion of thought in classical as in quantum physics. From classical physics such errors have not, however, been used to defend the rationality of 'belief' in free-will as have misinterpretations of quantum physics.

At this stage it is well to recollect that one of the basic

ideas of present-day research is that we cannot find out what things will look like or how they will appear to behave *solely* by thinking about them. If the highly logical methods of mathematics are used as tools of thought to tell something about what a human observer can observe, then a beginning from some of the known facts about human observers must be made. It is equally logical to begin by ignoring the known facts of human observation, but in this latter case there are no rational grounds for surprise if the logical thinking ensured by the use of rigorous mathematical processes leads to results which cannot be directly interpreted in terms of what research workers can observe in the laboratory. At one time it was thought that observation was unnecessary to determine if heavy bodies fell quicker than light ones, and that the matter could be settled solely by the use of logical reasoning. Galileo's use of observation in settling the problem must never be forgotten if it is wished to avoid confusion of thought when applying pure reason to problems of natural phenomena.

The second example taken from quantum physics is less obscure than Dirac's 'observable,' and is simpler to analyse. The Bohr-Heisenberg electron microscope* is used to illustrate a mathematical concept. In his chapter on "The Possibility of Measurement and Heisenberg's Relations" de Broglie expresses this concept as "that no observation is capable at one and the same time of determining a co-ordinate and its conjugate momentum with an accuracy greater than that expressed by Heisenberg's uncertainty relations." It is evident that the observation here discussed is not observation by a human being, for the discussion begins: "Let us consider a material particle, for example an electron. To determine its position with great accuracy we have only one means, and that is to employ optical methods; but these allow us to measure a co-ordinate only to an approximation of the order of the wave-length." We

* See Heisenberg, *Physical Principles of the Quantum Theory*, p. 21. 1930; L. de Broglie, *Introduction to the Study of Wave Mechanics*, p. 147. 1930; a gamma ray microscope is discussed by Condon and Morse, *Quantum Mechanics*, p. 22. 1929.

have seen that agreement between different observers can be reached about the position of, for example, a tiny drop of oil such as was used in Millikan's experiments by using judgment of coïncidence of the drop or a boundary of the drop with a cross-wire of a microscope. Such a method cannot be used with an electron so that the phrase "to determine its position with great accuracy" by the use of 'optical methods' refers to the mathematical determination of the position of an electron and not to some coincidence observations made by a human observer using a visual method. Heisenberg in the section dealing with 'Determination of the position of a free particle' begins by describing the use of a microscope to observe a material particle (such as a speck of dust) and the influence thereon of the resolving power of the microscope. He then continues with an imaginary case of something like a microscope to be used for observing a moving electron, and states, "But for any measurement to be possible at least one photon must be scattered from the electron and pass through the microscope to the eye of the observer." The illustration is used as an analogy between the explanation, by wave theory of light of the influence of the resolving power of a laboratory microscope upon the visual observation of a stationary tiny object by a human observer, and the imaginary observation of a moving electron by an imaginary observer. The phrase "the eye of the observer" does not refer to a human observer, for such an observer can see neither the stationary nor the moving electron. In discussing the observation of a moving electron Heisenberg adds to the postulates of the wave theory of light, one about an imaginary observer who could see an electron at rest. He then shows what it would be logically possible for such an imaginary observer to see of the electron in motion. The fact that the argument is logically sound makes its result no less imaginary than the original postulates. It deals with a mathematical problem and directly tells nothing of what a human observer can see of an electron at rest or in motion.

Attention is directed to the profound differences between observation by judgment of coincidence, as described in this

book, and the more complex process called measurement which involves more than coincidence observation. 'Observing the position of' does not mean the same as 'determining the position of.' The word measurement seems to be well on the primrose path which leads to philosophy as the following definition (or indefinition) shows. "The foundations of the quantum theory are the laws of measurement in atomic physics, i.e. *the general principles of the interpretation* of experimental observations made on the ultimate elements of the physical world."*

This detailed discussion of only two of the many points which arise in discussing the physical interpretation of quantum physics is given as an attempt to correct the false impression given to the general reader by some popular scientific writings and by the interpretations of some philosophers of certain results of mathematical physics. It is as true to-day as it was at the time of Galileo's experiments that sound arguments *alone* can tell nothing about natural phenomena. To give scientific knowledge of natural phenomena of the external world, as distinct from the world of mental concepts, the sound arguments must begin from observations made by human beings.

Summary.—Scientific knowledge is based upon observations made by the use of sense impressions. In order that the knowledge may be impersonal those qualities of human observers which are common to all men must be used as much as possible. Ordinary observation of everyday life gives no agreement between different observers. To reach such agreement only one method has been so far discovered and widely applied—it is the method of observing by judging coincidence or fit and the observations are called coincidence observations. The judgment of coincidence is not the simplest judgment in which agreement between observers can be reached, for agreement can be reached about judgment, for example, of one thing being between two others. In actual use in research the judgment of coincidence involves the judgment of one thing being between two others, and

* G. Temple, *The General Principles of Quantum Theory*, p. 22. London, 1934. Chapter ii deals with The Laws of Measurement in Atomic Physics.

probably the judgment of nearness to one or the other of the two objects. In all experiments judgment of coincidence is used dynamically, necessitating judgment of both fit and lack of fit. Apart from the introspection-observations of psychology the facts used in scientific research are such that they have been, or in the present state of knowledge could have been, obtained by the observation of coincidences. For the purpose of getting agreement between observers the use of measurement is quite unnecessary.

CHAPTER VII

PATTERN

In discussing the subject of ordinary observation it was suggested that the initial stage seems to be the perception of a whole. More generally it would appear that all sense data come first in wholes and are later analysed into smaller and smaller wholes ending (in scientific observation) with the simple whole consisting of two parts, between which coincidence or the lack thereof is judged. Observation of single isolated things is impossible, for a single object without a contrasting background is invisible. Some kind of whole, even though it be as simple as object and background, is invariably essential in either ordinary or scientific observation. In this chapter attention will be directed to this idea of a whole—which, for reasons given later, is to be called a pattern. The concept has already been used in the study of observation, and it is used later in the book in discussing the various ways in which scientists arrange or group facts (coincidence observations). The term pattern property will be usually understood to refer to any property which is characteristic of a whole, but not of the parts into which the whole may be divided for study.

SYMMETRY AS A PATTERN PROPERTY

Let us begin by getting as clear an idea as possible of the sense in which the word pattern is to be used. The reader is asked to perform a little experiment on himself. Look at Fig. 12, consisting of a square in outline, and think whether or no it seems to mean more than four straight lines of equal length forming a closed figure with right angles. Does not the mere arrangement of those four lines in that particular way seem to give a whole which is more than or different from the sum total of the separate units? In Fig. 13 the same four lines are used, but are arranged into a different pattern. In looking at Figs. 12 and 13 one does not seem to

notice first of all four straight lines of equal length, but rather a single thing, a geometrical figure in this case, or in general a pattern. It is only later that separate parts or features of the figure seem to be noticed. What elements of these two figures does the reader notice when he has become familiar with them? He is asked to pause here until some analysis has been made of what is noticed about the figures as a whole.

I am unable to make this experiment myself since I first seriously studied patterns in connection with crystallography and X-ray crystallography. The patterns connected with these subjects often have certain so-called elements of symmetry, and I cannot look at any object without at once, and almost unconsciously, noticing which of these elements of symmetry are present. The reader unfamiliar with crystallography may be interested to compare his analysis of Figs. 12 and 13 with the following which would almost certainly be made by any X-ray crystallographer.

Studies in X-ray crystallography have shown that the normal stable state of all solids is crystalline. On this view solids can be considered as built up of patterns of atoms, the patterns themselves being small units repeated many millions of times in the microscopic piece of solid called a crystal. Any one of these patterns could be described in terms of the distances of the individual atoms from the axes of a three dimensional co-ordinate system. This does not, however, express the pattern of the structures so well as an analysis in terms of the idea of symmetry. Here is an example of an element of symmetry. If in the structure or pattern, a plane mirror could be placed in such a position that the optical image of the part of the pattern in front of the mirror exactly fitted over the part behind the mirror, then the pattern has a plane of symmetry in the position of the mirror. In everyday life whilst the word 'symmetry' may be used to describe form, it is often used to refer to an object having a plane of symmetry. The Victorian mantelpiece was an example, with its clock in the middle and its pairs of ornaments placed equidistant from the clock and on each side of it. Gothic church windows

are other good examples of objects having planes of symmetry.

In the analysis of Figs. 12 and 13 and of their pattern, it is probable that the planes of symmetry of Fig. 12 were noted. They are perpendicular to the paper and pass through the lines HI, GE, DB, and AC shown in Fig. 14.

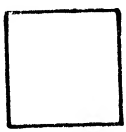

FIG. 12

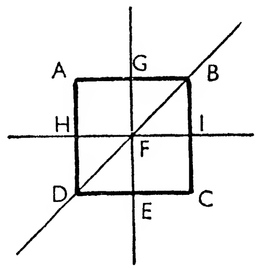

FIG. 14

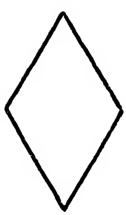

FIG. 13

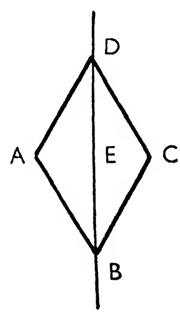

FIG. 15

For Fig. 13 the corresponding lines are DB and AC. These may all be regarded as ways of getting one part of a figure to fit over another part. In X-ray crystallography other ways are also used. Studies are made of axes about which figures may be rotated, in order to make one part of the figure fit exactly over another part. If Fig. 12 is rotated about an axis perpendicular to the plane of the paper and passing through the mid-point F of Fig. 14, then four times in each complete rotation the figures would be indistinguishable from Fig. 12 as drawn. The figure is said to have a fourfold

axis of symmetry. If it is rotated about the axes GE,] DB, or AC then twice in each rotation coincidence obtained. These are twofold axes of symmetry. Fig. 13 fewer of these axes of symmetry, two corresponding to and AC of Fig. 15, and one through the mid-point E, p pendicular to the plane of the paper. Incidentally it wo be of interest to know if this second way of getting cc cidence between different parts of a geometrical figure, t way of describing some of the properties of a patte naturally occurs to the reader who has not stud crystallography.

The examination of these two simple geometrical figu has shown that the putting together or grouping of a num of separate things may produce something, called here pattern, which in these cases would be readily recogni: as a pattern. Some properties of the pattern could be reac analysed or described in terms of types of symmetry. S more important to note is that these same properties w not seen as inherent in the individual units. The patt taken as a whole has properties which do not seem consist of the simple addition of properties of the individ things from which it is built. Take away one line fr Fig. 13 and we take away not one quarter of the symme properties discussed but the whole of them.

From this it is evident that a number of things conside as a whole may have properties which cannot be descril as a simple addition of the properties of the separate un The mass of four uniform separate 10 grm. weights tal together is 40 grm. which is the result of the simple addit of their separate masses. The same four weights resting the bench can, however, be grouped to form many differ spatial arrangements. Mass has simple additive propert pattern has not.

PATTERN PROPERTIES IN PHYSICS AND CHEMISTR

An atomic nucleus and six electrons may be a collect of positive and negative electricity or it may be an at of carbon. An atom of carbon is not regarded as the sa

as any other collection of the same amount of positive and negative electricity. Positive and negative electricity arranged in that particular way seems to have properties or meaning for us which cannot be seen as a simple addition of the properties or meaning of positive and negative electricity considered alone. Huge numbers of the same carbon atoms which are regarded as identical individuals, when put together in one way, give a picture of charcoal or graphite. Put together in another way they give diamond. Not only the individuals but also the way they are put together, the pattern into which they are arranged, seems to matter in considering some properties. The clearly defined allotropic modification of some of the chemical elements and polymorphic crystalline substances are examples explained as the same things arranged in different patterns. The same elements of biochemistry may be built into a gibbering idiot or into a Faraday. Iron in a handful of nails is not equivalent to the same quantity of iron in the haemoglobin of the reader's blood.

In the subject of chemical combination a vast number of substances can be regarded as the same numbers of the same elements put together in different patterns to give compounds of the same composition, as determined by chemical analysis, but having widely different properties. The subject is dealt with in text-books of chemistry under the name of *isomerism* when it is classified into three types. In speaking of the chemical elements the first type, polymorphism, has already been mentioned. In the substances calcite or Iceland spar and aragonite, the common elements calcium, carbon, and oxygen, of common chalk are taken as arranged in different spatial patterns to give crystals of quite different properties. As an example of the second type, polymerism, the two substances ethylene C_2H_4 and butylene C_4H_8 may be cited. These were also (historically) the first pair of organic compounds to be recognized as of identical composition, but they have nevertheless many different properties. Although two molecules of ethylene have the same number of carbon and hydrogen atoms as are contained in one molecule of butylene, the two substances have

different densities and behave quite differently. Faraday cited them as examples "of bodies composed of the same elements and in the same proportions but differing in their qualities." His prediction that "now we are taught to look for them they will probably multiply on us" has been amply fulfilled with the development of structural chemistry from about 1858 onwards. The following three compounds have the same common formula $C_3H_6O_2$

Ethyl formate $H.CO.O.C_2H_5$
Methyl acetate $CH_3.CO.O.CH_3$
Propionic acid $C_2H_5.CO.OH$

but they have quite different properties and they cannot be converted into one another by any simple chemical process. A change of pattern alone without a change of type of atom can be used to explain different properties of the substances. Since their molecules contain the same numbers of atoms, they are better examples of isomerism than is the example cited by Faraday. Reference to Richter's *Lexikon der Kohlenstoff-Verbindungen* (Leipzig, 1910), where compounds are indexed under their numerical formulae, shows remarkable examples of this isomerism. For example in the first volume on pages 366–7 are indexed *seventy-one* compounds which have the common molecular formula ($C_6H_{12}O_6$) of glucose, but each with different properties corresponding to the particular way in which the atoms are arranged.

The special senses in which *pattern* and *pattern-property* are used can now be expressed more clearly. In detailed examination of actual objects and events attention is concentrated on various selected aspects. Some of these selected data can be expressed as a kind of addition of parts, but some cannot be so expressed. Pattern properties are any selected properties of objects or events which cannot be recognized as inherent in the properties of separate parts, even when separate parts are clearly discernible in the object or event under examination. The word pattern is used because it seems to express the ideas that there is something to be taken as a whole, but that this whole is

built up out of parts clearly discernible as separate. Pattern also seems to involve the idea of separate parts fitting into some larger whole. The idea of a certain man-made arbitrariness which the word implies is considered later on, more especially in Chapter XI. The words organization or design do not seem so suitable because they seem to suggest the idea of planning or arranging for some unified purpose, and they seem to emphasize unduly the whole. The word pattern seems to suggest equal importance of the parts and of the whole and will be used in that sense.

Consideration of some everyday examples of pattern will serve to illustrate the extended meaning of the word. It is an easy step to the patterns of fabrics, and it will be a convenient one to enable us to discard the idea that a spatial pattern must have some simple geometrical symmetry. Many fabrics now in use can be seen to have patterns built up by the repetition of one unit or motif (corresponding to the unit cell of X-ray crystallography), but this unit itself has none of the usual elements of symmetry. Symmetry is not an essential part of a pattern in the sense in which the word is here used; neither is repetition of an observed regularity. The idea of symmetry was used at the beginning of the chapter to show that even at the present time man has found one way of discussing certain pattern properties. To insist that some selected property of an object is a property of a whole, does not therefore necessarily place it outside the scope of further scientific study.

The idea of pattern properties in science is not new. One of the earliest books dealing especially with problems of scientific method as we now know it, is Robert Boyle's *The Sceptical Chemist* published in 1661. Chemistry was then still largely alchemy. The following passage taken from the book, besides giving some striking examples, illustrates that the Boyle known to physicists as the originator of one of the gas laws, was also a keen student of scientific method. "To show how slight a variation of textures without addition of new ingredients may procure a parcel of matter divers names, and make it be looked upon as different things: I shall invite you to observe with me, that clouds, rain, hail,

snow, frost, and ice may be but water having its parts varyed as to their size and distance in respect of each other, and as to motion and rest. And among artificial productions we may take notice . . . particularly of the sugar of lead, which though made of that insipid metal and sowre salt of vinegar, has in it a sweetness surpassing that of common sugar, and divers other qualities, which being not to be found in either of its two ingredients, must be confessed to belong to the concrete itself, upon the account of its texture."*

PATTERN IN LITERATURE

The general reader may prefer examples taken outside laboratory studies. In literature verse-rhythm is an example of a whole having properties which cannot be seen as inherent in the separate parts.

> "The curfew tolls the knell of parting day,
> The lowing herd winds slowly o'er the lea,
> The ploughman homeward plods his weary way,
> And leaves the world to darkness and to me."

The general effect of this stanza from Gray's *Elegy in a Country Churchyard* is one of pleasant drowsiness, yet this same effect cannot be seen as inherent in the parts. If analysis be applied many factors helping the effect can be noted. First of all is the simple metrical pattern of × / × / × / . . ., etc. When this is broken in the second line by 'winds,' the break serves to lengthen the line. When it is broken at the end of the fourth line, it is to give a *diminuendo* ending to the stanza. The lines are all prolonged by the abundance of long, open vowels. In the first line the effect of tolling bell is helped by the liquid ll's. There is a general binding together of the whole stanza by the alliterative effect of the ten *l* sounds, of the t's, w's, d's, and the's. Yet when the use of all these separate devices has been noted, no explanation of the general effect is thereby obtained. The same devices are used in other poems to give other effects.

* Everyman's Library Edition, p. 203.

So far as dictionary meanings are concerned, a passage of verse can be expressed equally well in prose.

> "Up to a hill anon his steps he reared
> From whose high top to ken the prospect round,
> If cottage were in view, sheep-cote or herd;
> But cottage, herd, or sheep-cote, none he saw."

These lines from the Second Book of Milton's *Paradise Regained* may fairly be translated:

"Thereupon he climbed a hill on the chance that the view from its summit might disclose some sign of human habitation—a herd, a sheep-cote, a cottage perhaps. But he could see nothing of the sort."* The general effect or 'meaning' of the two is, however, quite different. Even when the same words are used they are placed in a different environment, or whole, in the two passages. Both passages have their own verbal patterns. In the verses there is a highly organized interplay of rhythm, emphasis, order, and vowel-consonant combination. These are much less highly organized in the prose but the differences are at present quite inexplicable.

The 'bare sheer' poetry of Wordsworth shows that poetical words and complex inversions of language are not essential elements in poetry.

> "A slumber did my spirit seal;
> I had no human fears:
> She seemed a thing that could not feel
> The touch of earthly years.
>
> No motion has she now, no force;
> She neither hears nor sees;
> Rolled round in earth's diurnal course,
> With rocks, and stones, and trees."

The only poetical word in these lovely lines is 'slumber,' and only two simple inversions of the prose order of words

* See Lecture iii, "On the Difference between Verse and Prose" in A. Quiller-Couch's *On the Art of Writing*. Cambridge, 1916.

are used. Of the forty-eight words only one has three syllables, six have two syllables, and the remaining forty-one are mono-syllables. Language and rhythm are of the simplest. Yet the poem is regarded as one of Wordsworth's greatest triumphs in investing personal feeling with unusual significance.

Some modern poets use rhyming patterns showing no simple regularity and some do not use rhyme at all. Some write lines showing no regularity of accent or metre. Some of the more 'advanced' poets give up the arrangements of words used to convey rational meaning and make patterns of sounds. Nevertheless literary critics agree that some kind of formal pattern is to be found in all works of literary 'merit.' The devices of balance between words, or lines, or paragraphs, of antithesis and antiphonal effects, almost Biblical in their nature, are still widely used in 'free' verse.* The scientific reader who thinks that he has 'grown out of poetry' or has 'no use for it' may be reminded, as an observed coincidence, that a number of research workers do indeed enjoy poetry and furthermore some actually write it. If man is a creature of pure reason it is difficult on first thoughts to understand how anyone, much less a scientist, who wishes to convey meaning of any sort, should make the attempt with verse. But it is a fact that words arranged in the form of verse mean to us something different from their use in prose. Pure reason fails when we try to understand this particular human activity. On abandoning the idea of man as a pure reason machine the activity may seem 'natural,' and there are no rational grounds for surprise that in some circumstances Clerk Maxwell builds that beautiful pattern called the Electro-Magnetic Theory of Light and in other circumstances fits words into a pattern to give the merry jingle of *A Problem of Dynamics*:

"An inextensible heavy chain
Lies on a smooth horizontal plane,
An impulsive force is applied at *A*
Required the initial motion of *K*

* See Bullough, *The Trend of Modern Poetry*, 1934, especially chapter iv dealing with the Imagists.

> Let *ds* be the *infinitesimal* link,
> Of which for the present we've only to think;
> Let T be the tension, and $T + d\,T$
> The same for the end that is nearest, to B."

and so on.*

The pattern idea is readily applicable to all kinds of poetry but is more difficult to discuss in other forms of literature, in the novel and the play. If repetition of a unit be not insisted upon in pattern, and if emphasis be laid upon the fitting of separate units into a whole, and of the separate units thereby acquiring qualities or significance which they have not when considered separately, then those novels and plays which are called good have pattern. Details about the author's people and incidents are not enjoyed unless they fit into some kind of aesthetic, emotional, or dramatic scheme. This whole scheme may not be realized until the end of the novel or play has been reached. On reading the book for the first time interest and enjoyment may be got from the beauty of the language, the wit, and from other details, but still more enjoyment is got when these details are seen as fitting with a certain inevitability into some whole. Neither all writers nor all readers display this quality. To mention only one trivial detail, in a novel the colour of a man's eye may change from blue to brown and some readers do not notice the change. But it is a common feature of those novels and plays which literary critics call good, that the separate parts have not only an interest or beauty of their own, but that they seem to acquire an added interest or beauty or significance by their place or fit in the general scheme.† Critics sometimes comment on the inevitability of the flow of events in a good novel or play. No one part more than another can be said to cause this quality of good literature. It seems to depend on the totality of the parts.

* Campbell and Garnett, *Life of Maxwell*, pp. 404–6. London, 1884. Several of his poems are there printed in an appendix, pp. 383–401.

† For detailed analysis of aspects of Shakespeare's technique in producing this effect, see Bradley, A. C., *Shakespearean Tragedy*. London, 1915, or Moulton, R. G., *Shakespeare as a Dramatic Artist*. 1885, Oxford. (A popular illustration of the principles of scientific criticism.)

An analogy can be seen between the idea of pattern, or fit of electrons into atoms or of the atoms into the unit cell of a crystal to explain certain properties, and the idea of pattern or fit of the words, ideas, events, or actions giving to poem, novel, or play its literary style or effect. In the biological sciences it has long been recognized that many of the problems relate to the fitting of parts into a whole, and the organic chemist has for the past seventy years used the pattern idea in the development of structural chemistry. Mathematical crystallography is based upon the pattern concept, and current mathematical theories of the atom are designed to take account of the relations of parts to a whole by using theory of groups.

PATTERN IN MUSIC*

Although perhaps music is less widely appreciated than is literature, it contains so many examples of the pattern idea that I cannot resist the inclination to mention some of them. A tune or melody is a pattern. If a phrase of a well-known tune such as "God Save the King" or "Three Blind Mice" be sung or played and then repeated at a higher pitch, either version is at once recognized as the same tune. Yet an entirely different set of notes may have been used. The second set of notes may have frequencies quite different from those of the first and yet the two wholes, which are formed by putting two quite different sets of things together, are said to be the same in one respect; they are the same tune.

The next point is slightly more technical. If a phrase be played in two keys so chosen that at least one of the notes is used in both versions of the tune, then although from an acoustical point of view that note is the same, yet from a musical point of view it seems quite different in the second version. To take a particular case. Play on the piano the three adjacent white notes named *e d c*. They give the opening phrase of the tune "Three Blind Mice." Now play *f*♯, *e*, *d*.

* The reader who is not interested in music may omit this section without loss of essential points in the discussion of pattern.

These also give the same phrase in a different key. There is no denying that the acoustical effect of playing the notes *e* and *d* is the same in each case, and yet these same acoustical sounds seem quite different in the two cases. In either phrase a whole is recognized; a part of a tune, something not essentially inherent in the individual notes. Also the same things, the notes *e* and *d* seem to acquire a different meaning according to the whole or pattern into which they are fitted.

Similar phenomena are met with in harmony in the study of chords or simultaneous combinations of notes. Play together the notes *g b d f* and you have a dominant seventh chord in the key of *c*. Play only the same *g b d* and you have a common chord of the key of *g*. The first chord does not seem like the second with something added to it. It seems to be a whole in which the same separate parts have acquired a difference according to their environment. For a musician the first whole or pattern called the dominant seventh chord is more like the chord *g*♯, *b*♯, *d*♯, *f*♯, having no notes in common than it is like the common chord of *g*, in spite of the fact that this latter has three notes out of four in common with it. The musical physicist will be able to appreciate this to the full as an example of one whole, built up from quite different parts, being more like another than is a third having in common three out of four parts separately identical. Much more striking examples can be given where one may use the technical language of harmony.

As Browning puts it in *Abt Vogler* " . . . out of three sounds he frame, not a fourth sound, but a star." Not only in single notes played in succession to give melody, and in notes played together to give chords do we get these patterns or wholes having properties different from the sum total of the separate units. The subject studied as 'musical form' gives many more examples. Canon, fugue, rondo, sonata form, and the variation, are patterns. Many compositions may have quite different contents and yet all have the pattern called a fugue. A fugue or any other of the many musical forms is a pattern or a whole not essentially dependent upon the character or nature of the individual notes used to make it. A piano has eighty-eight notes. It is not

differences in the individual notes but the way they are p together into patterns that makes the character of a Sca latti sonata, a Bach fugue, a sonata of Haydn, Mozai Beethoven, or Brahms, or a prelude of Chopin or Debuss The properties of the units alone do not adequately accou for the properties of the wholes into which they are bui In considering the subject of music there is then abunda evidence of the usefulness of the pattern idea.

MEASUREMENT *VERSUS* PATTERN

The term pattern property is here used to mean any proper of a whole which is characteristic only of the whole ai not of parts into which the whole may be divided. Patte properties seem to depend upon the totality of parts rath than upon their simple addition.

The use of the pattern idea has been most frequent in tl biological sciences in which the study of separate pai without relation to the whole has, by some, been condemne On the other hand most writers on the so-called exa sciences have emphasized that way of studying Natu which involves studying parts of wholes by the use measurement. This process seems to consist in calling oi bit of Nature a measuring rod and another bit of Natu an object or phenomenon to be studied. The one is p by the side of the other and judgment of coincidence used to compare the changes in one with the suppos constancy of the other. It has been claimed that the resultii metrical knowledge is the one and only kind of scientil knowledge. These two ways of looking at Nature have l some been regarded as rivals. Metaphorically speaking, tl physicist's measuring rule has been regarded as somethii with which to flog to death organized wholes or patterr Scientific research has, however, now got to the stage wh both these points of view are wanted to study the kind things in which scientists are interested in both biologic and non-biological sciences.

One thing is certain, man is so made that he finds if not essential, at least much easier to study things by fi

splitting them up. Such a process as abstraction (Chapter IX) is indispensable in research technique, but can be used to study either metrical or pattern properties of Nature and both aspects are needed in scientific studies. If only metrical aspects are considered the research worker gets a collection of facts. He may be able to see order in these selected facts, giving classifications or natural laws (Chapter X). Whether or not he is successful in this task, he is not satisfied until he has got some whole or pattern, a scientific theory (Chapters XII and XIII), into which to fit the facts.

THE STUDY OF PARTS IN BIOLOGICAL SCIENCE

The fruitfulness of studying parts of living organisms as distinct from the whole organism is apparent from the development of biochemistry. At first attention was directed to static aspects of living things, as, for example, when Wöhler in 1828 first synthesized urea outside a living organism. This type of research in which substances usually made inside living things are made outside of them, in purely chemical apparatus, was apt to be criticized by some biologists on the grounds that it dealt, not with living but with dead things. It did not show that living things were unessential to the production of organic substances, for living chemists were needed to manipulate the apparatus and chemicals. But it showed that these organic substances could be made quite outside of the living things. Encouraged by these results biochemists have directed attention to more dynamic aspects of living things and have shown, for example, the remarkable part played by vitamins and hormones in the processes of growth and action. These processes are very characteristic of living organisms as a whole, and the results illustrate how studies of parts help to an understanding of some of the characteristics of the living thing as a whole.

In biochemistry the selected part of the living thing is in character very different from the whole organism. There are, however, other striking types of experiment illustrating that the behaviour of a part of a living organism when

separated from the whole animal may not be wholly unlike its behaviour in the animal. One type of example is in tissue culture *in vitro*, when a part of the animal is grown in a glass tube instead of in the animal itself. Especially remarkable are the results of Strangeways and Fell.* In one set of experiments they removed limb-buds from fowls' eggs which had been incubated for 72–80 hours and cultivated these tiny fragments in glass tubes containing suitable nourishing media, changing the media every two days or so. The fragments would at first consist of similar living cells and would be quite unlike a chicken's leg. The cells of these fragments continued to multiply as in ordinary tissue culture, but after about two days they could be seen to begin to produce cells of more than one type. Later on cells characteristic of cartilage, bone, and horny material appeared, and after three weeks something like the leg of a chicken in an egg incubated for that period was obtained. The purely physical conditions would be different from those of legs growing inside incubated eggs and the legs grown in glass tubes would vary, for example, according as the medium was more or less liquid. Nevertheless, specimens grown in glass tubes quite separated from the whole egg embryo could be obtained so like chickens' legs that one would not think of calling them anything but legs.

As a physicist I was very surprised to see, in a biochemical laboratory in another type of experiment, the heart of an animal, completely removed from the animal and connected up to a special apparatus, continuing to beat rhythmically for several hours. In such laboratories this perfusion of the heart experiment is commonly used as a means of testing the influence of drugs. The heart is connected to tubing so that Ringer's solution at a suitable *steady* (not a pulsating) pressure and temperature, can be slowly passed through. The heart continues to beat and the experiment may be continued "for at least three hours without causing any

* "Experimental Studies on the Differentiation of Embryonic Tissue growing *in vivo* and *in vitro*." *Proc. Roy. Soc. B.*, vol. 99, pp. 340–66, 1926 and later papers. A cinema film of some of these experiments has been prepared by Canti and shown to many scientific societies.

marked change in the tone, excursions, or rate. The threshold and the response to vagus stimulation also remain unchanged during this time."* Similar results are obtained with portions of intestine completely removed from the animal. It does not follow that because the heart retains in these experiments its characteristic rhythmic beating when removed from the body that therefore it behaves in all other respects as if it were still connected to the animal. Nevertheless the results of these experiments serve to caution a non-biological thinker against adopting the view that only the whole or pattern idea is of service in scientific studies of biological material. The method ending in coincidence observation and measurement and also the pattern method are both needed and have already proved to be of service.

THE STUDY OF WHOLES IN NON-BIOLOGICAL SCIENCE

Turning from biological to non-biological sciences it is evident that again both of these ideas are needed. In chemistry the quantitative laws of chemical combination are a result of using the measurement idea whilst structural chemistry is a result of using the pattern idea in which the relation of parts to a whole is foremost. In classical physics the pattern idea was found to be of great service in dealing with all kinds of electrical circuits. No matter how much *individual* electric cells or condensers or 'resistances' or 'inductances' are measured, we cannot get enough knowledge to enable us to deal with combinations of those into wholes called electrical circuits. Remarkable differences are found in the behaviour of the same components of a 'radio' set when they are connected together in different ways. The behaviour of collections of electrical batteries and condensers, for example, depends not only upon their individual properties, but also upon their arrangement in the electrical circuit. Even if attention is confined to metrical knowledge it is found that, for example, the electrical resistance of two

* Sollmann and Barlow, *J. Pharm. Exp. Ther.* 29, p. 233. 1926. For details see such a text-book as Sollmann and Nanzlik, *Introduction to Experimental Pharmacology*. Philadelphia, 1928.

equal coils of wire taken as a whole may be either more than or less than the resistance of either of them taken separately. The technical use of the phrases 'in series' or 'in parallel' describes pattern ideas which enable us to deal with the combinations of resistances, or condensers, or batteries very simply. Classical mathematical crystallography is based essentially upon the idea of relation of parts to a whole, that is upon the pattern idea. It is logically sound and it fits the facts so that although it is essentially non-metrical mathematics it is as scientific as any purely metrical study. In atomic physics a stage has been reached in which purely metrical ideas have been found unsuitable for dealing with certain experimental results of measurement. It is now apparent that metrical ideas suffice for some problems, but that the pattern idea of relation of parts to a whole is needed to deal with other problems, which may be called problems of the organization or totality of the atom.

It is noteworthy that the scientific devices of measurement and of cause-and-effect relationship seem to be unsuited to express pattern properties. Even the most detailed list of measurements does not express such an idea as symmetry or any kind of shape or form, and it does not seem helpful to speak of any one part more than another as causing the symmetry or form. Pattern properties are non-metrical and non-causal.

In a quite different field it is now being found that some problems of civilization can be dealt with individually, but that others are essentially 'pattern' problems which so far as may be seen at present, can be dealt with only by studying the relationship of parts to a whole. The vigorous use of the Should-Ought Mechanism in dealing with this type of problem does not help the solution, although it may provide some psychological comfort to the user.

CHAPTER VIII

ARE FACTS FIRST SEEN IN ISOLATION?

SINCE all scientific facts are the special kind of sense data called coincidence observations, it follows that scientific knowledge of even such inanimate things as are studied in physics and chemistry, must depend in part upon qualities of biological material, that is it must depend upon qualities of human beings. On this view, using also for convenience the hypothesis of a world external to the observer, scientific knowledge depends partly upon qualities of the external world and partly upon qualities of the observer, neither of them to be considered apart from the other. Our study has so far revealed that one of the properties common to all men is their power to judge coincidence or lack of coincidence to a degree of fineness depending upon the individual and upon the sense used. The degree of fineness necessary to tell the time indicated by a watch is adequate for much scientific research and the layman is as well able to do this as is the laboratory worker.

This power can be used to give the facts, but as Poincaré has put it "a collection of facts is no more a science than a heap of stones is a house." When scientists indulge in the unscientific activity of assessing values they may value a scientific theory as highly as a new fact and in practice arranging facts is an essential part of research. The next task is then to find what methods are used in scientific research for grouping facts together. If scientific research is regarded as a form of human action some quality or qualities of man which enable him to group facts together or to deal with large numbers of facts must next be sought.

Does observation give nothing but isolated facts which are then put together by the application of pure reason or logical thinking about them? There is strong evidence in favour of the view that observation by human beings has another quality of great importance both in scientific research and in everyday life. This quality is the direct

perception of wholes, quite apart from any conscious reasoning about separate isolated features of the object examined, and quite apart from any rational 'meaning' which any part of the object may have for the observer. Some of the evidence has been given in the chapter on Pattern. Here, some other features of observation will be presented and discussed.

INFLUENCE OF FAMILIARITY

It was seen that one essential factor in observation was attention and that to be sure of observing some feature it is desirable both to look at, and look for it. But newness is often an essential feature of an object or a phenomenon studied in scientific research. If knowing what to look for is necessary to the making of reliable observations how can a research worker observe something which is quite new to him? Will he see in it only those parts or things with which he is familiar or which are rather like something he has seen before? If such considerations were followed to their logical conclusions it would be puzzling to know how a start could be made in observation. Objects and happenings which are now very familiar must at some time in the individual's life have been quite new and unfamiliar to him. If it be supposed that he is born with some innate knowledge of the external world it might become necessary to suppose that the whole of what he sees of the external world is born in him.

What then is the influence of familiarity on observation? The reader is asked to test himself. Without reading beyond the end of this sentence glance at Figs. 16–22, at the same time making internal observations of what is first noticed about them. Does one first notice the figure as a whole or does one in Figs. 16 and 22 first notice hexagons or in Figs. 20 and 21, squares? These are familiar geometrical figures, but the diagrams as a whole are so simple that it is perhaps difficult to tell what is first noticed about them. Now in fact the diagrams were drawn by first making a very clear letter and by then adding lines without altering the letter

at all. For example, Fig. 16 originated in letter H, Fig. 17 in A, Fig. 18 in L, Fig. 19 in E, Fig. 20 in M, Fig. 21 in T, and Fig. 22 in W. These letters in the very clear form in which

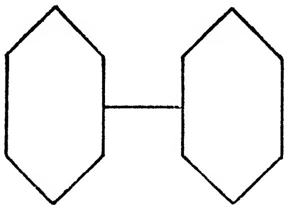

FIG. 16

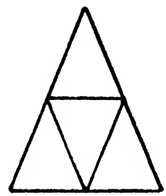

FIG. 17

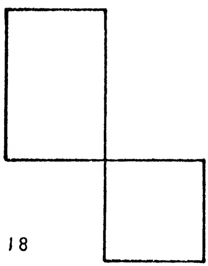

FIG. 18

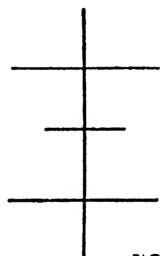

FIG. 19

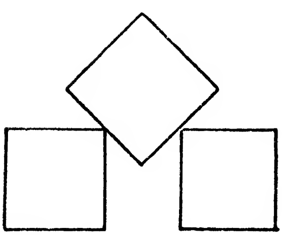

FIG. 20

FIG. 21

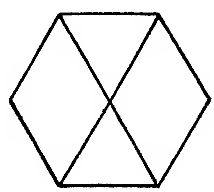

FIG. 22

they occur in the diagrams are far more familiar than are hexagons or squares, and yet these very familiar objects are not noticed first. Note the delay in observing that Fig. 22 contains X and V, Fig. 21 both T and L, and Fig. 19, E, F, T, L, I, and H. It is evident that in observation of a new

object those features about it which are already familiar are not first noticed. The figure seems rather to be taken in as a whole. The analysis and concentration upon certain details seem to come later, and in this later process previous knowledge and experience play a part. For example, Fig. 16 suggests to me a formula in organic chemistry, and Fig. 21 a diagram to illustrate the kind of condenser used by Hertz in his experiments on electro-magnetic waves.

Another example of initial failure to recognize the familiar in an unfamiliar environment occurs in looking at puzzle pictures such as were used in children's magazines. A black and white sketch would be given of, say, a pond and drawn in unusual positions would be perhaps three ducks. The puzzle was to find the three ducks. Although the observation of this type of puzzle and of Figs. 16–22 shows that the familiar is not always noticed first, it does not show that a whole is necessarily observed first. It is still reasonable to suppose that some kind of rapid synthesis of a whole is made.

To decide between a rapid synthesis and direct observation of a whole, a simple experiment with an amusing toy can be used. This toy* consists of a postcard having an outline of a head from which a part is omitted. The gap is filled by a thin flexible chain whose outline continually varies as the card is moved about. The surprising fact is then that the appearance of the *whole* figure changes with each change in the shape of the chain. The knowledge that the greater part of the figure remains constant, does not alter this change in the appearance of the whole when only a part of the figure is changed. If the first stage of observation is a process of seeing details and rapidly synthesizing these into a whole, it is difficult to understand why in using this toy a different *whole* is synthesized with each change of the chain. The first stage of observation seems rather a process of perceiving a whole, which is not synthesized by the use of pure reason, or of reading 'meaning' into what is seen, or of utilizing previous experience. Judg-

* One form of the toy is sold as the Facio-Graph, Pro. Pat. No. 4288/25.

ments of coincidence are not initial observations made upon unfamiliar material.

This idea of the initial direct observation of a whole and of a change of one detail altering the appearance of the whole seems to help the understanding of a number of well-known facts. For example, beauty is evidently a property

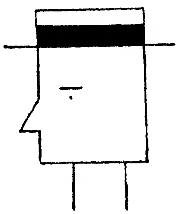

FIG. 23

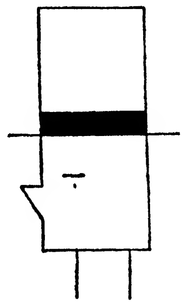

FIG. 24

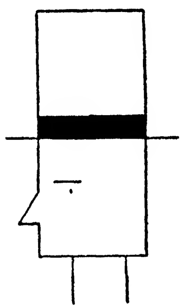

FIG. 25

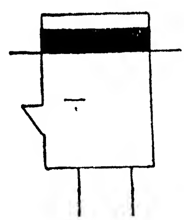

FIG. 26

Figs. 23–26.—Diagrams to illustrate changes in a whole produced by altering any one detail. Compare any pair of diagrams differing in either nose or hat, i.e. 23 and 25; 23 and 26; 24 and 25; and 24 and 26.

of observation of wholes. The skilful tailor or dressmaker can make a man or woman look less fat or thin, tall or short. Examination of Figs. 24 and 26,* in which the two faces are

* As the figures can be drawn with drawing instruments the identity of the faces is easily secured. For conventional drawings of the same woman's head with two different styles of headdress see Diagrams XXI and XX, pp. 180–1, of H. Speed, *Drawing*. London, 1913.

the same, shows that alteration of headwear alone can alter the appearance of the face. Recognition of the instance cited in the toy postcard is shown by the proverb 'Circumstances alter cases, just as noses alter faces' (Figs. 23 and 26). Horizontal stripes make a woman look fatter, vertical stripes make her look thinner than do plain fabrics. The change in the whole appearance of a man's face when he shaves off his moustache or when he adds a false moustache or beard, or the change in the whole appearance of a woman's face produced by a change in style of hairdressing or of hat are widely familiar. It is not always easy at the first glance to recognize people dressed quite differently from when they were introduced, although their faces are unchanged. The architect can alter the whole appearance of a given building of constant size and shape by varying the number, size, shape, and arrangement of the windows even if the total area of glass is kept constant. The whole appearance of a room may be altered by altering the decoration. Heavily patterned walls make a room look smaller than if its surfaces are plain.

If the idea of initial perception of a whole is used there is no difficulty in understanding the initial observation of things so new and unfamiliar that they have for the observer no recognizable similarity with anything in his past experience and no rational 'meaning.' A start in the making of observations is possible without postulating inborn or acquired knowledge.

In the next stage, if a new thing is examined for long enough it ceases to be unfamiliar. What happens in the stage of becoming familiar with some stationary object? In the first glance at a collection of 'meaningless' dots the individual dots are not noticed as such. In the next stage, the whole first perceived is divided into groups. In one part of the figure four in a straight line, in another part three at the corners of a triangle, and so on. Similar observations can be made by looking at the sky on a clear night. The stars seem to be observed in groups depending upon such factors as brightness and position quite apart from what the observer may know of astronomy.

It would appear then that observation of a stationary object begins with observation of a whole and continues with subsequent observation of subsidiary wholes or groups. In Chapter V reference was made to the seeing of three dots not as three individual dots, but as three dots in a triangular group. On the present view then we see the wood first and then the trees. There is no logical compulsion to postulate the use of pure reason, or of familiarity or of 'meaning' in order to see groups of facts as wholes. Sense data seem to be perceived directly in groups. The complication seems to arise in the subsequent analysis and selection of parts of the sense data. The difficulty is not to see what scientists do with facts when they have got them, but rather to understand what determines the selection or abstraction of individual facts from the wealth of impressions ever flowing in from the environment. The facts chosen often seem to be in themselves trivial and not such as at once arrest the attention. This is a problem to be solved not by discussion but by experiment, that is, by controlled observation and such a study calls for a thorough knowledge of biology including psychology.

PERCEPTION OF CHANGE

As to this selection from a wealth of sense data, consider now the observation of an object which is familiar but in which some change occurs. It is, I think, common experience that the change is at once noticed. In watching a gas discharge tube through which an electric current is passing whilst the tube is gradually evacuated, a glowing column may be seen similar to that in neon advertisement signs. As the gas is pumped out of the tube the appearance of this column changes in a complex way, details of which are given in text-books of physics. An observer is at once conscious of the change even if he has never before seen the experiment. On the present view the observer first sees a whole in which he notices nothing in particular except perhaps the colour of the light. As soon as the gas pressure is reduced to such a stage that the column breaks up into

a number of striations he at once notices the change. If a non-scientific observer is shown this with no warning whatever of what to look for, he at once asks what is happening, showing that he notices a change in the appearance without expecting it, and without knowing what to look for.

Similar experiences are found in everyday life. Although individuals differ widely in their powers or inclination to perceive change most people notice changes in the rooms in which they live and work, and in the towns and countryside with which they are familiar. The differences between individuals reveal one very significant fact, that sometimes the individual notices a change *without* being able to tell what detail has been changed. This seems to support the view that one quality of human observation is that a *whole* is directly observed, and is not synthesized from the separate details which are later noticed as observation is continued. Otherwise if the whole is built up from the same kind of details which are later observed, it would be impossible to notice a change in a complex whole, *without* noticing which particular detail had changed. It would therefore appear that man is so made that he can directly perceive *change* or a difference, as such.

If two photographic negatives which are nearly but not quite the same are placed one over the other, and are moved slightly out of register, the region where the different detail occurs is at once noticed. Makers of flashing and other moving advertisement signs use this fundamental property of human observation; that one sure way of attracting the attention is to make a change in the environment. The flashing signs of Piccadilly Circus are almost a danger to the pedestrian crossing the road there because they rival, in the pedestrian's attention, the changes which are taking place on the road.

This perception of change seems to be one factor which may determine upon what part of a whole attention is directed after the initial glance. As it refers to noticing a change occurring during observation it is therefore involved in the observation of moving objects. It refers also to a

change, occurring in the absence of the observer, and subsequently noticed on re-examining the object. Individuals differ widely in this observation of change. I came across one case of a man failing to notice that a room which he frequently used had been repapered, and it was not quite certain that he noticed any difference even when his attention was directed to the subject. In this case the two papers were similar, but to most people they would have appeared distinctly different if in no other respect than that the new paper was uniformly clean where the old one was soiled in some parts more than in others.

Wolters* refers to a series of experiments in which observers were shown in succession pairs of cards, each for a quarter of a second, and were asked to say whether the second card bore the same design as the first. In most pairs the difference, if any, lay in one element only. "When the comparison card varied from the standard only by a slight modification of some detail, the difference was generally overlooked, although the detail in question had been properly noted on the first card." This result seems to differ from my own experience in the spontaneous observation of changes in rooms with which I am familiar where I seem to note instantaneously any change in the region at which I glance. Perhaps the small time of one quarter of a second, and the use of sequences in Wolters's experiments, account for the difference.

The following points relating to the direct and spontaneous observation of change refer partly to what I happen to have noticed with my own experience. Firstly, the observation of change is not primarily related to assessment of value or of *interest* in any ordinary meaning of the word. I note changes such as the difference in wood polished with and without the use of polishes, or the difference in appearance of a carpet with the pile lying in a direction different from that when I last saw it, whereas I do not consciously attach the slightest importance to such things. For weeks I note a picture not parallel with the edge of a book case, without having any particular views on how

* *The Evidence of our Senses*, p. 71. London, 1933.

the picture should be hung, but the first time the frame is set parallel, I note the change. This perception of change without conscious interest seems to be a basic element in observation. Secondly, the observations seem to be made without attention being given to the specific thing observed. To cross a room to get a book from a book-case probably involves enough attention being given to a piano to avoid a collision, but nevertheless changes in the details on the piano are noticed, although they are not sought and although one's mind seems to be occupied in thinking of other things. Thirdly, the noted changes in details may be so minute or trivial, that they would be omitted from a description of the original appearance of the object. It seems as if the memory preserves something like a photographic negative of a very familiar scene. At the next examination this memory image is unconsciously placed over the visual image present, and, just as with two similar photographic negatives, attention is immediately attracted to the places where the two do not exactly fit, that is, to the places where there is a change in one relative to the other. It is noteworthy that this remembered whole cannot always be recalled to memory so as to enable details to be described. Nevertheless changes in these details would be noticed. On revisiting a place after several years' absence, have you not recognized that some building now there, was not there on your last visit, and yet have you never failed to remember what was originally in the place of the new building? The memory image seems to be a whole, not a collection of details. Fourthly, I have noted no lack of change perception of minute details after periods up to several months. Fifthly, I have noted that the phenomenon is aided by good lighting and is comparatively weaker in rooms which are dimly lighted.

The perception of change seems to be a property of all of the sense organs, for changes of sound, taste, smell, and temperature are readily noticed. In travelling in a rapidly moving vehicle changes in the motion are noticed by aid of the kinesthetic sense. By touch a difference in accidentally putting on the wrong hat, or in picking up by mistake

a tennis or badminton racquet which looks at first glance like our own is at once noticed. It may almost be said that a continuous sound such as an electric motor or an X-ray set in operation is 'heard' only when it stops or when the sound changes. An interesting example of the use of this phenomenon in a famous piece of research is told of the late Lord Rayleigh by his son. When Rayleigh was doing the classical experiments which led to the discovery of argon, it was necessary to spark mixtures of gases for long periods to get rid of all chemically active gases. To get even a small quantity of the chemically inert gases the sparking had to be continued for long periods "prolonging the work into the night. . . . The hammer break of the coil was liable to stick, so that constant watching was necessary. Rayleigh had a telephone arranged so that the hum of the induction coil upstairs in the laboratory was transmitted to him as he dozed in his arm-chair in the book-room. If the noise stopped, he woke with a start, and went up to set the coil going again."*

Facts and arrangements of facts were given in Chapter I as two essential qualities of scientific research. This study of observation as the source of all facts has revealed a new problem. Instead of finding that in observation facts come singly it has been found that the initial stage of observation seems to be perception of a whole. A study must therefore be made of those factors influencing the selection from the whole, of the details which are later called individual facts. These selection factors will be discussed in later chapters. The direct perception of change is only one of many which may influence concentration of the observer's attention upon some detail of the originally perceived whole. Although attention was earlier directed to finding a technique of observation which would give agreement between different observers, our study has shown that observation itself contains two elements of the greatest importance in all intellectual work. In the language of vision these two elements may be expressed as the seeing of individual details selected

* *Life of Lord Rayleigh* by his son, p. 198. London, 1924.

from a whole or from a pattern, and the seeing of a whole or pattern itself without giving attention to the individual details as such. These same two elements seem to be like the derivation of a particular case from a general relationship, and the recognition of a general relationship between a number of particular cases. This alternation between the general and the particular, which has been recognized as a feature of normal human observation, is of the greatest service in scientific research, as indeed in all intellectual work. One is tempted to describe the application of pure reason as in part the putting of things into certain patterns, and the selecting of certain individual things from patterns. In such an analogy it seems that one man's patterns may be another man's chaos.

In summarizing it may be said the initial stage of all observation appears to be direct perception of a whole and is followed by division of this whole into subsidiary wholes. In this sense the observation of relationships comes *before* the observation of parts or of individual facts. The basic facts used in science have to be selected from the wholes in which they are first observed. They are not first seen in isolation. Observation is not a synthesis of those individual details which are later noticed, for a change or difference, as such, may be directly perceived.

CHAPTER IX

SELECTION AND ABSTRACTION

It is one of the properties of man that if he tries to give attention to many things at once he becomes confused. Confusion of thought is a hindrance to scientific research and the research worker must of necessity concentrate attention upon some minute detail of Nature. Scientific research, such as, for example, a scientific theory of the universe, which at first sight seems to be far from concentration upon minute detail, proves to be so when examined more carefully. It may well be found that although a theory of the universe tells much of things many millions of miles away, it nevertheless tells nothing of human beings or of such things as the 'simple' daisy of the field. There is no exception to the rule that scientific research never tells all about anything no matter how minute the detail dealt with by the research.

Nature appears to be one unified whole. The minutest detail can be seen by man to have some relation to its environment. Some of the properties of an object may be lost by removing it from its initial environment, and it may acquire new properties by being put in a new environment. To understand how a scientist comes to select certain facts rather than others is far more difficult than to see what he does with the facts when he has got them. Some attention will therefore be given to distinctly human factors involved in the selection of scientific data although it must be admitted that little is known of the subject.

Consideration of certain of the facts discussed in the chapters on observation suggests that the initial stage is the perception of a whole. At the first glance nothing in particular is noticed, but within some fraction of a second after observation is started, the observer begins to concentrate attention upon detail. Unfortunately for the student of research technique little or nothing is known of the factors which determine this selection of detail from a whole. Little order can be seen at present in the factors which influence

interest. Why one man is interested in chemistry, another in music, and a third in both, as was Borodin, is a complete mystery. It is consistent with the general attitude of this book to acknowledge that physiological factors must play a part in influencing interest. The state of a man's liver may be as important an influence upon what he notices as may his so-called philosophy of life. It is common experience that a hungry man pays more attention to those details in his environment which relate to the sights, smells, and sounds of meal preparation than does a man who has just dined. In the variation of this physiological factor lies an element which is, at the same time, different for each observer, and which furthermore in the same individual varies with time. In asking in what details of his environment a particular man is interested, the occasion when the observations are to be made must be specified. Those factors in the individual which influence the selection of detail from a given external environment may be referred to as his internal environment and will include not only physiological factors but mental factors.

Considered broadly, internal and external observations go together and what is selected seems to depend as much upon the internal environment of the observer as upon this external environment. As a physicist I do not try to regard what happens in an electric network, when it is connected to a battery, as depending upon either the battery or the network taken alone but upon the two considered as a whole. The scientist's selection of data is here regarded similarly as a property of the whole formed by the scientist in his environment. Because of my lack of knowledge of the physiological factors influencing selection of material for study, I can discuss the influence of only a part of the internal environment of the observer, and shall consider in particular the influence of past experience.

INFLUENCE OF PAST EXPERIENCE

One important factor affecting the observer's choice of detail from a whole seems to be his store of past observations.

The same objects seen more than once by the same individual do not look in every way the same. The internal observations and the details of sense data upon which attention is concentrated are not the same when I return to a sunlit, primrose-decked wood in springtime after having visited, seen, heard, and smelt a sordid city slum. Each new symphony I hear slightly alters what I notice on rehearing the old ones. A very crude mental picture of observation by an individual may be formed by supposing that each observation is registered upon something like a cinema film, and that in making the next observation, the whole of this succession of negatives is flashed through the mind of the observer. What he then selects from the initial whole and registers upon the film depends upon what is already upon the film. The film registers not only sights but sounds, smells, and all other sense data. The analogy is quite crude, and must not be regarded as a physiological mechanism. It would be more in keeping with the attitude of this book to suppose something like a composite picture rather than a succession of separate impressions. The analogy is offered to help the understanding of a number of puzzling facts about the individual scientist's selection of data.

For example, it often happens that a scientist notices what seems to be a very trivial and dull thing. Some facts such as lightning, earthquakes, eclipses of the sun and the like are very dramatic and grip the attention of all. These phenomena are studied by scientists, but are exceptional. More research is done upon things which to most people look remarkably dull and uninteresting. It seems difficult to understand how anybody could ever have looked twice at them. Scientists do not select only the spectacular. Moreover, the selected facts are not such as can be understood only by the pioneers who originally selected them. A degree student in a university has little difficulty in observing facts which have remained hidden from man for centuries and whose selection represents the life-work of many scientists. Subtlety, obscurity, and incomprehensibility are found not in the selected material, about which all scientists agree, but in the interpretation of the facts. This disagreement

between scientists is not about the selected detail but about the theories and other devices into which the facts are fitted. Once a detail is pointed out, many men have no difficulty whatever in seeing that which before they did not see.

To understand how certain facts come to be selected from the vast amount of available material and then only by particular individuals on some particular occasions, it may be supposed, as a working hypothesis, that individual observers differ physiologically, as do all biological individuals, and also that each is a synthesis of his past experience. From the beginning and throughout life observation will depend upon biological or physiological factors, and also upon what has already been observed by the individual. Adam and Eve and Newton and millions of others have seen apples fall. Observation of the fall of an apple by Newton was not the same as observation of the same phenomenon by Adam because each was biologically unique and because the integration or synthesis of past observations for each was different. What is observed depends not only upon what is there to be observed, but upon the observer, and upon what he has previously observed.

I will frankly confess to the reader that I do not like one implication of this hypothesis.* I should like to think that of those data of physics, which in the next few years will be selected by various research workers in regions to which I have access by the necessary laboratory facilities, I have the same chance of myself being the selector. On a simple so-called mechanistic point of view the conditions would be apparently the same or very similar for each research worker. On the present view the conditions are not the same, for no two research workers are more like than are two peas in a pod, and although similar things are often said to be as like as two peas in a pod, no two peas are ever so alike that they cannot be distinguished as different.

With so complex a biological object as man, there are

* Although the hypothesis is incompatible with that of free-will it implies nevertheless that the complete set of observations of each individual is unique, differing in some respects from the complete set of any other man. Free-will is not the only hypothesis explaining individuality.

wide differences between individuals. Similarities can be seen between different observers, but in the end it must be admitted that Michael Faraday did what he did, because he was a biological unit and was therefore unique; no other man would be expected to do precisely what Faraday did. The ideas of a 'standard research worker' and of '99 per cent pure sodium chloride' can be conceived. The latter can be found in any chemical laboratory, the former nowhere. Each research worker is unique and his selection of data is unique. The common property of research workers, that they can all judge coincidences, gives a test which must be applied to their selected data before it can be accepted as scientific knowledge. It does not provide the data in the first place.

PERCEPTION OF SIMILARITY

When an observer's environment changes he retains something of the original sense impressions and seems to fit them over the new sense impressions. He at once notices changes or lack of fit of the new with the old to a degree of fineness depending upon the conditions, but where the time available is long enough he notices also something like fit or similarity. He can select from the new whole, things which seem like something he has seen before. He can see a tomato, an apple, a plum, and a cherry and at once notices differences of form, taste, smell, and feel, but he can also notice a similarity which he calls redness. Seeing in turn grass, lettuce, cabbage, and peas he can select a similarity, greenness, of colour. Neither redness nor greenness nor colour can be directly observed. Only the coloured objects can be seen, but from the observations the idea of colour can be abstracted. This process of selection by noticing similarity gives the abstractions used in thought. It is a process used as much in everyday life as in research and seems to depend upon fitting past experiences over present experiences and noticing in what way they fit. It does not seem to be a process independent of past experience.

Seeing similarities seems more difficult than seeing dis-

similarities. The attention is at once attracted to any change in an object, but not so readily to any similarity of the object with some other. Pictures in the fire or in clouds are not seen at the first glance although they come suddenly when they are seen at all. The similarity seen by the chemist between a fire, breathing, and rusting, or between coal and diamond, are not apparent at first glance and depend very much upon past experience.

One way of testing the seeing of similarities and of differences, is to show an observer a pair of objects and ask him to compare them. Individuals vary very much in the proportion of similarities and dissimilarities they mention. In extreme cases an observer may be found who records no similarity between a glass of water and a glass of milk, two objects which for many observers would be very similar. When the similarities are pointed out, abnormal observers can at once recognize them. Their past experiences are adequate to enable them to recognize the likenesses which nevertheless do not occur to them.

With the co-operation of some honours physics students to whom I was giving a course of lectures on scientific method, the following experiment was carried out before I had said anything in the lectures about similarity and difference. Two objects were placed upon the lecture bench and the observers were asked to compare them. The words used were: "I will show you a pair of objects and ask you to compare them. Please write down the first things which enter your head relating to the two objects; put down, say, up to six points concerning them in the order in which they occur." The pairs of objects were in turn (1) a micrometer screw gauge and a measuring cylinder, (2) a closed box of filter papers and one of X-ray films, (3) a watch and a thermometer, and (4) a glass beaker and a small part of a piece of research apparatus whose function was known to no one but myself. Two minutes were allowed for each of the tests (1) to (3), and two and a half minutes for the fourth test. Ten observers took part and recorded a total of 148 points of comparison. As soon as the written answers were collected I asked in what sense each observer had

understood the word compare. One of the observers thought that only points of similarity were to be noted, this being his own interpretation of the word 'compare.' Each of the other nine understood that both similarities and differences were to be noted. I had been careful not to mention similarity or difference before the test or in the previous lectures. Of the 131 points noted by the other nine observers, 111 related to differences between the two objects and 20 to similarities. This result seems to favour the spontaneous noting of differences rather than of similarities even in a two-minute examination of objects. The experiment would have to be repeated on many more observers before detailed analysis of the results would be justified. It may, however, be noted that almost all the points of comparison seemed to fall into six classes. The numbers of points recorded in each class were as follows: (1) Shape or form including position on the bench—36, (2) Use or function—31, (3) Size —18, (4) Material—24, (5) Colour—20, and (6) Distinctive or functional marking—17.

Another way of studying the selection of detail from complex wholes is by the analysis of language. The making of a word to name a thing follows sooner or later upon the selection of the thing from its environment in which it was first observed. The making of such a word as *food* illustrates that some similarity has been seen in articles as diverse as fish, meat, game, fruit, and vegetables. The invention of each of these words shows recognition of similarities for both salmon and sole are fish. Moreover, 'salmon' cannot directly be observed or eaten, this can be done only with some particular salmon in some particular environment. This mental grouping together of things separately recognizable as different, but in which some resemblance is seen, is the process of classification. It is an essential element in the rational use of, or reaction to, one's environment. The seeing of similarities is as necessary to life as the seeing of differences, but the former process seems to be more difficult.*

* It is interesting in this connection to note that some primitive tribes have no words for abstract ideas of either quality or kind. The aborigines of Tasmania have words for each variety of gum-tree,

There seems, however, to be no end to the resemblances which can be seen as observations are multiplied. The lack of cocksureness in the man of knowledge is often accompanied by his seeing similarities in the subject matter under discussion, with other facts of which the cocksure man is ignorant. Nevertheless there is another element in not seeing similarities; man has the power of not seeing or of refusing to recognize similarities, where he does not want to see them. Wants come before the use of pure reason, and the ardent patriot who fails to see any similarity in his own country's acquiring of colonies and the attempts of another country to do likewise, is often in the state of not wanting to see the similarity. The man, lacking a sense of humour, who does not see similarities between the behaviour of the inmates of a Zoo's monkey house and his own behaviour, is often not indifferent to the subject. He definitely does not want to see such similarities. The occurrence in conversation of such phrases as 'But that is quite different' or 'I do not see the connection' is sometimes a sign of unwanted, rather than of false, analogies. The scientific study of human behaviour is largely hindered by this type of factor. In the past the older sciences have begun with classification. Classification is a process of recognizing similarities and any obstacles to such recognition are obstacles to scientific research. The scientific study of human behaviour is difficult not only because many separate factors can be selected for study, but because some fruitful analogies are unwelcome.

Dislike rather than lack of fit is a factor in some criticisms of analyses of behaviour in terms of concepts which do not include consciousness or free-will. To what extent likes and dislikes affect the selection of details for study in scientific research or the perception of similarities is but little understood. Men intellectual enough to do scientific research, have necessarily the mental powers enabling them to

wattle-tree, etc., but no equivalent for 'a tree.' The Zulus have words for 'red cow,' 'white cow,' etc., but none for 'cow.' The Mohicans have words for cutting various objects, but none for 'cutting.' See Jespersen, *Language*, pp. 429–31. 1922.

rationalize any of their actions if they wish to do so. Reasons given for choice of material may then be rationalizations invented after the choice was made.

SOME TYPICAL ABSTRACTIONS

In analysing research technique there is considerable difficulty in deciding in what order to present the selected factors. Discussion of any one factor necessitates reference to others which have not previously been discussed. Research technique like pure reason is a unified whole. The name covers actions which are superposed in such a way that their isolation and presentation in succession may give a false idea of the way research work is done. In practice there is a continual alternation of various devices. A piece of research does not progress in the way it is 'written up' for publication.

Two ideas are in perpetual use in research. They are the idea of concrete objects of the laboratory, i.e. objects of the hypothetical external world, and abstract ideas or mental concepts formed by studying these objects. The distinction made between the two is that the observer becomes aware of the concrete objects by seeing, touching, hearing, smelling, or tasting. The concrete objects can be observed by the coincidence method. The observer can also be aware of the abstract ideas or mental concepts but he cannot manipulate them with his hands. Mental concepts are not observable by the coincidence method.

It should be noted that no attempt at all is here made to devise any kind of explanation, such as a scientific theory, to account for the differences of which the individual is aware. Such ideas as, that concrete objects 'exist' outside the observer or that concrete objects 'exist' as one kind of mental impression inside the observer are not necessarily involved in this discussion. The reader is given a test (coincidence observation) which he can apply to tell whether something of which he may be aware is or is not to be called a concrete object. The test is to be taken as a definition of concrete object, based upon the idea of agreement

between human observers. The test is not an assumption about the 'existence' or 'reality' of concrete objects.

In internal observation the observer may be aware of either concrete objects, or abstract ideas, or emotions, or pain. Except in some physiological or psychological literature, scientific statements are made in terms of concrete objects and mental concepts only. As action in the laboratory or in everyday life is often accompanied by consciousness of abstractions, a few typical examples will be discussed.

Consider first the idea of a straight line. A line ruled on Bristol board with indian ink and a special ruling pen would be called a straight line. A stationary plumb-line with a very fine thread or wire hangs in a straight line. The edge of a well-sharpened razor is straight. Examination of any of these objects with a microscope of suitable magnification shows an irregular outline where formerly no irregularities were seen. But an irregular outline is no property of a straight line, neither is a straight line made of indian ink and cardboard or of thread or wire or steel. On careful examination, all the objects said to be straight have a host of other properties which have nothing whatever to do with the idea of a straight line. The straight line is an idea which can be got by seeing many different objects. From each is selected something similar and common to all, and temporarily no attention is given to the other dissimilar properties. The idea of a straight line could not be got by looking at a single black line ruled on a sheet of cardboard, for we should not know from this single example that a red line might be a straight line or that a straight line might be horizontal or vertical or in any other position. For seeing similarities and getting abstract ideas many examples must first be examined. A rich background of knowledge is for this reason very helpful in the practice of research work.

If the idea of a circle is similarly considered it is evident that no circle drawn on paper or metal and no circular object have solely the properties of the mental concept of a circle. All circular objects have irregular outlines when examined in detail and they are all made of some definite substances, whereas the abstract idea of a circle has neither

of these properties. If similar considerations are applied to the oval curve called an ellipse, we shall then have three typical scientific abstractions and can use them to illustrate further abstraction.

One property of an ellipse is that two fixed points called its *foci* can be chosen such that the sum of the two distances of any point of the ellipse from the foci, is constant. For a very oval ellipse the two foci are far apart. If, however, the two foci be supposed to get nearer and nearer to each other, the ellipse would look less and less oval until when the two foci coincided the ellipse would be what has been called previously a circle. Once this has been pointed out a circle can be regarded as a special kind of ellipse and the one abstract idea of the ellipse can be made to serve the purpose of two. I do not know what other ideas are involved in seeing this relationship between an ellipse and a circle, so that I do not mean to imply that a total of only two concepts is used in seeing them as different, and one in seeing them as one. At any rate only one geometrical concept is needed to cover the two when this reasoning has been used. We can go a stage farther by noting one property of the circle, that all points of it are the same distance from a given point, the *centre* of the circle or the two coincident foci of the ellipse. Suppose now that this distance, the radius of the circle, gets larger and larger. A small piece cut from a very large circle would look very like a short straight line. The larger the radius the more nearly would the piece look like a straight line. Ultimately it would be quite impossible to detect any lack of fit with a straight line. By this means a straight line can come to be regarded as a part of a circle having a huge radius.

This very simple example of abstraction in which certain common similarities have been selected from many objects and have been formed into the abstract ideas called respectively, straight line, circle, and ellipse, is very characteristic of research technique. The example is worthy of very serious attention by the non-scientific reader who enjoys reading popular expositions of the results of scientific research. To help clarity of thought it is vital to note that the abstract

ideas are got by deliberately neglecting a vast amount of information which is temporarily irrelevant. If we want to think only of a geometrical concept it is a waste of effort to take any account of the magnetic properties of the steel straight-edge or steel sphere, but it by no means follows that those properties will always be irrelevant even in scientific research. Boltzmann put it thus: "It has never been doubted that our ideas are merely images of the objects (or rather symbols for them) which have a certain relationship with the objects, and never completely correspond to them, but are related to them as letters to sounds or notes to musical tones. Also on account of the limitation of our intellect they are able only to depict a small part of the objects."*

When the abstraction has been made it can be manipulated only with the mind. In the laboratory an object can never be found having the properties of our abstract, without having also a vast number of other properties which are completely ignored in making the abstract. Furthermore, in the mind, all sorts of things can be done with these abstracted ideas which cannot be done in the laboratory with the objects from which the abstracted qualities were first got. A mathematician can work with triangles, but a physicist has to use triangular pieces of wood or metal or the three feet of a spherometer and the like. I see no rational grounds for being surprised when it is found that a laboratory steel-rule does not behave always in exactly the same way as do the mental concepts derived from it and used in the mind. In the very expectation that a few mental concepts abstracted from examining some object or phenomenon will be able to tell us 'all that matters' about the thing, we seem to be giving ourselves credit for remarkably penetrating vision. If it be assumed that other concepts are trivial, then, in effect, either Nature or other men's interests are being criticized. In research, as also in everyday life, very unpleasant surprises are apt to be met if a few mental concepts which happen to have served well in past actions are regarded as 'all that matters' in dealing with objects or events from which the mental concepts were derived. A

* *Vorlesungen ueber die Principe der Mechanik*, vol. i, pp. 1–2.

clear idea of the views here given on the simple geometrical abstract ideas of the straight line, circle, and ellipse will be of great service in later discussion of hypothesis and theory, where mental concepts which cannot be visualized at all may be used. For the understanding of the results of scientific research the realization of a distinction between the abstract and the concrete seems to be far more necessary than a distinction between abstract ideas which can be visualized and those which cannot.

The *particle* is another very characteristic abstraction used in both classical and modern physics. Its use in dynamics where interest is centred upon objects in motion, may be illustrated by the following definitions. Kelvin and Tait state, "we have taken no account of the dimensions of the moving body. This is of no consequence so long as it does not rotate, and so long as its parts preserve the same relative positions amongst one another. In this case we may suppose the whole of the matter in it to be condensed in one point or particle. We thus speak of a *material particle*, as distinguished from a *geometrical point*."* Haas states: "To simplify the following investigations we shall assume that the body moves as if its whole mass were concentrated at a single point. The following considerations are therefore based on an imaginary so-called *particle* . . . this imaginary concentration of the mass has the advantage that we can disregard all forces which might act within a body, all changes of shape, and all eventual rotations."† These definitions state quite clearly that certain qualities of objects have been deliberately ignored, and it is found in practice, that agreement between observations and calculations based upon considering only selected qualities of moving objects can be reached. For clarity of thought it is necessary to remember that the abstract idea of a particle is obtained by selecting only very little from the examination of many different objects. Logical reasoning is then applied to this abstract idea in the mathematical devices of dynamics and the result may tell something about the motion of the

* *Treatise on Natural Philosophy*, Part I, § 215, p. 219.

† *Introduction to Theoretical Physics*, vol. i, pp. 16–17. 1924.

particle. To relate this to objects in the laboratory, the logical reasoning must be expected to tell us something only about the motion of laboratory objects. It is found in practice that results can be got about, say, the motion of some laboratory object by reasoning about the mental concept called a particle, but this does not mean that the laboratory object is a particle. When some man of genius such as Newton selects a few qualities of many different objects and from them forms certain abstract ideas and names them, it does not follow that his particular selection contains all of the properties of the objects that matter to human beings or even to physicists.

It is useless to search laboratories in the hope of finding particles. From the description of the particle of dynamics it might be thought that a tiny steel ball such as can be found in a ball bearing was something like the particle of dynamics. The steel ball could be picked up with a magnet, but not so the particle of dynamics. Only the steel ball would reflect light, and only the steel ball would rust if left out in the rain. A novice in dynamics could get an idea of a particle by imagining the steel ball shrunk more and more, without losing its mass, but he would still have to put out of mind a host of other properties of the steel ball before he had grasped the idea of the particle of dynamics. Neither a tiny drop of oil such as was used in Millikan's experiment, nor a drop of water such as is seen or photographed in a Wilson Cloud Chamber, nor the much smaller objects such as are studied in bacteriology, are particles. No human observer has ever seen a dynamical particle. It can be found only in the mind of the scientist. This distinction between what can be observed by human observers and what can be conceived in the mind, is here laboured in reference to the dynamical particle, because this latter can be very readily visualized in spite of the fact that it cannot be seen. Once this idea is grasped, that the particle of elementary dynamics cannot be seen, one is less worried by the fact that another kind of particle called an electron, which cannot so readily be visualized, cannot also be seen.

In some scientific work mental concepts are used which cannot be visualized. This double complication, firstly that they are mental concepts and cannot therefore be observed, and secondly that they cannot be visualized, has led to some remarkable paradoxes in popular science literature where some of these difficult mental concepts have been treated as if they have the properties of concrete objects.

When thinking of the use of the particle in dynamics another abstraction readily comes to mind, that of the *simple pendulum*. It is said that Galileo when a youth noticed that a lamp pendant hanging from the roof of the cathedral in Pisa swung in the same time, as judged by his pulse, no matter what the range of the swing. If the range of variation of swings is not too great a similar fact can be noticed for any other similar swinging body. From these diverse examples a selection can be made. Notice need not be taken of the fact that Galileo saw a lamp, and that it happened to be a lamp in a cathedral in a town called Pisa. Thousands of other properties of the particular objects and events present with Galileo and with all other observers who watch objects swinging can be ignored. A particle can be imagined to be suspended by an imaginary thread from a fixed point. To such an idea certain dynamical and logical rules can be applied, and it can be deduced that the time of swing of such an imaginary system would be independent of the range of the swing, provided this range was not too great. Here is an abstract idea, a mental model which fits certain facts abstracted from the observation of swinging objects. The mental model is called a simple pendulum, but if the same term is applied to a brass sphere suspended by a fine thread, then the same name is given to two things which differ in ways, several hundred of which could be thought of as quickly as they could be written out. If the term simple pendulum is given to the mental concept, a full description of which can be found in text-books of dynamics, then the hanging lamp in Pisa Cathedral is not a simple pendulum. Neither are the concrete objects consisting of metal spheres suspended by fine threads and used in physical laboratories. Galileo's lamp could upon occasion

be used to light the cathedral, but no dynamical single pendulum could be used for such a purpose. In a physics laboratory the metal spheres used as the 'bobs' in simple pendulum experiments, can be, and often are, used upon other occasions for experiments on density or on specific heat, but the particle of the dynamical simple pendulum could not be used for such purposes. From this it is evident that even in the same branch of science, one property of a concrete object may be selected for study upon one occasion, and a different property of the same object upon some other occasion. Even in physics one selected abstraction is not necessarily the only one which matters to the physicist as such.

In parenthesis attention may be directed to two points. Firstly, the particular selection made by Galileo is another example of each observer's selection being unique. There is little doubt that millions of men with pulses had looked at swinging objects before Galileo looked at the cathedral pendant lamp at Pisa. He had himself probably seen the same lamp swinging on other occasions. But other occasions were not the same, differing for him both in his physiological conditions, and in the observations he had previously made. From a purely physical point of view, Adam and Eve or any of Galileo's predecessors or contemporaries might have been expected to notice what Galileo noticed. Assessment of value cannot account for the failure of his contemporaries. They used sand-glasses to measure time, and would therefore assess as valuable a pendulum device which could be used similarly.

The second point arising out of the discussion of typical abstractions and of interest only to those who teach physics or mechanics is that these very simple examples are admirably adapted to the teaching of this fundamental aspect of scientific method, that a scientific study of any subject necessitates a *selection* of material, and that any selection leaves temporarily out of account a vast number of facts which, for some other purpose than the one immediately in hand, may be of the utmost importance. As I do not use the Should-Ought mechanism I do not say or imply

that elementary science should be so taught, but if it were so taught then two results might follow. Firstly, a good deal of mental confusion might be avoided in helping the realization that to form a clear mental concept, abstracted or otherwise obtained, and to give a name to that concept does not create something with all the properties of a concrete object. Secondly, since all scientific work starts with selection, no one scientific study of an object or an event can possibly tell man everything about it which man can get to know even by scientific study. Each of the sciences selects different types of properties from the same objects and events. The only research work in which the biological sciences can give no information is that kind which does not need one or more human beings.

The abstractions so far discussed are so simple that they are taught in those schools where mechanics is studied. The next one is slightly more advanced although it is still taken from elementary dynamics. It is evident that many objects which move are far from small, and it would be surprising if the mental concept called a particle, which is suited to studies of very small objects, were also suited to the study of large objects in motion, such as a swinging gate or a rolling ship. To study the motions of these the abstraction known as a *rigid body* is useful. Its properties illustrate how one concept which has proved useful in some studies may be incorporated into another mental concept, which is better adapted to studies of different objects. Haas's definition of the particle has already been given. The following extract gives his description of the rigid body and incorporates a clear statement of distinctions which are apt to be neglected or unstated. His statement is clear enough to require no further comment.

"In our considerations of the motion of solid bodies we may . . . (substitute) . . . the conception of a system of discrete particles for that of the single particle. At the same time we attribute a special property to this system, which corresponds to the most important property shown, in greater or less approximation, by actual solid bodies. This arises from the fact that the form and internal distribution

of mass in solid bodies vary in general so little during motion . . . that such variations can be neglected for the purposes of many dynamical investigations. We accordingly define a rigid body as a system of particles whose mutual distances are invariable." He gives the following footnote: "The fact that we can in this way obtain a *picture* of the mechanical behaviour of solid bodies, which is sufficient for many purposes, has nothing whatever to do with the question of the molecular structure of matter. It is, indeed, quite unnecessary that the picture should correspond to the reality: it is sufficient that the results deduced therefrom more or less exactly agree with actual observations."*

One more abstraction, the *string* of Acoustics, may be mentioned. Although Rayleigh was writing for the advanced scientific reader, he applied his usual caution and took the trouble to state that it is an ideal body, that is, one to be found in the mind only. "The 'string' of Acoustics is a perfectly uniform and flexible filament of solid matter stretched between two fixed points—in fact an ideal body, never actually realized in practice."† The only strings which can be observed are such as the steel wires in pianos and the catgut strings of the violin.

The abstractions here discussed in detail are very simple. But abstractions are used as much in everyday life as in scientific research, and many of those used daily are highly complex. For example, the idea of a *normal man* is an abstraction which, while very complex, is nevertheless quite easy to visualize. It would be simple to write a list of a host of qualities of a normal man. Each of the items in the list would have been the result of the examination of many individual men. They would not be fanciful qualities which had come into mind from an unknown source. Yet when the list had been made it would be quite impossible to find any one man who had all the qualities set down in the list. If the idea of the abstract concept had already been grasped, there would be no surprise in the failure to find even one normal man anywhere. He would not be

* *Loc. cit.*, p. 82.

† *The Theory of Sound*, vol. i, p. 170, 1926 edition.

sought, for abstract concepts are to be found only in men's minds, and they cannot be observed externally.

As much confusion of thought can be, and often is, produced outside the laboratory in such fields as politics, ethics, religion, and social studies, by failure to distinguish between abstract concepts and concrete objects and events. The mental confusion then necessitates continual use of the Should-Ought Mechanism and of Assessment of Values to deal with the misfits.

MENTAL CONCEPTS AND ABSTRACTIONS

To end this chapter a brief note may be made of the distinction between abstractions and mental concepts in general. All abstractions are mental concepts, but all mental concepts are not abstractions. Eddington writes: "We are accustomed to think of a man apart from his duration. . . . But to think of a man without his duration is just as abstract as to think of a man without his inside. Abstractions are useful, and a man without his inside (that is to say, a *surface*) is a well-known geometrical conception. But we ought to realize what is an abstraction and what is not."* The continuation of the passage makes clear that the distinction, to which importance is attached, is not that between the abstract and the concrete, but that between abstract conceptions and unfamiliar conceptions. In this book no special distinction is made between conceptions which are either abstract or familiar or unfamiliar or which can or cannot be visualized. They are all rather loosely grouped together as ideas which cannot be observed in the laboratory or elsewhere by human observers of flesh, blood, and other biological materials. They can be used in thought, but even if they can be visualized they cannot be seen. To me abstractions seem to be mental concepts whose source is readily apparent whilst at least some other mental concepts seem to be very far removed from daily experience. Redness would be called an abstraction because concrete red objects, red buses or flowers or books or ink or sunsets and the like,

* A. S. Eddington, *The Nature of the Physical World*, p. 53, 1929.

can be seen almost anywhere. An n-fold continuum would be called a mental concept, but not an abstraction, because the source of the idea is not readily apparent. It seems to be a problem for the biologists and psychologists to tell us to what extent we are 'fancy free' in making mental concepts. I do not feel at all sure that a man who had not formed some idea of the abstraction called three-dimensional space, would ever form the mental concept of n-dimensional space. For the purpose of doing experiments in the laboratory or of interpreting the results the origin of ideas or mental concepts does not seem to matter provided that they are obtained. When more is understood of how men get ideas, the knowledge will help research workers, but at present little is known of the subject. I wish to be quite non-committal upon whether or no all mental concepts are originally derived from experience of external objects. Dreams occur when the fancy seems to be free, but the contents even of dreams appear to be derived from observation of external objects. We seem to be able to abstract certain impressions and augment them, building them into new patterns and sequences.* For the purpose of practical research work and the interpretation of results, the distinction between observations and all mental concepts is more useful than that between abstracted and non-abstracted mental concepts. As already explained there is no suggestion that other mental processes are not involved in making observations.

* For a detailed analysis of the contents of a famous dream in terms of the external observation preceding it, see Book IV of *The Road to Xanadu: A Study in the Ways of the Imagination*, by J. L. Lowes. London, 1927. In this masterly study of Coleridge's external environment before writing *Kubla Khan* (and *The Rime of the Ancient Mariner*) Lowes maintains a thoroughly scientific respect for facts. Scientific readers may prefer to read the chapter on "Mathematical Discovery" in Poincaré's *Science and Method*. London, 1917.

ARRANGING SCIENTIFIC FACTS

"*Science is built up of facts, as a house is built up of stones; but an accumulation of facts is no more a science than a heap of stones is a house.*"—POINCARÉ, *Science and Hypothesis*, p. 141.

CHAPTER X

ORDER, LAWS, AND CLASSIFICATION

To understand the scientific or Natural Law and such elaborate scientific classifications as, for example, the periodic classification of the chemical elements, two special ideas seem to be necessary. Firstly, that of the abstract concept which was discussed in the previous chapter and, secondly, the idea of order.

ORDER

Scientific research is as much concerned with collections of facts as with the establishment of isolated facts. In his essay *De indagando vero*, published in 1779, T. Bergmann wrote, "A vast number of observations without order or regularity is not unlike a confused heap of stones, lime, beams, and rafters requisite for constructing an edifice, but which being combined with no skill fail in producing the proposed effect." In the process of abstraction, groups of facts are already used, for many objects must be examined before even such a simple abstraction as the particle can be made. Ordering is also a process needing more than one fact. It is therefore another tool to be used in dealing with collections of facts.

The idea of order seems to originate in those simple judgments which men can make and in which they can reach agreement. In making judgments of fit and lack of fit we seem to be using together a number of elementary judgments which are used individually in ordering objects or facts. If three dots lie in a straight line most men are so made that they could very well tell that one of the three dots was between the other two. If the dots had been made with crayons of different colours so that each could be readily referred to by its colour, one could at once say that, for example, the red dot was between the other two. Now it is evident that this property of the red dot is not something inherent in the red dot. It is necessary only to rub

out the other two dots to find that the red dot has lost this property of being between two other dots, even though nothing has been done to the red dot itself. It is evident that the property of between-ness is a property of the red dot only so long as it forms part of a whole or pattern. In making the judgment the red dot is seen as in some way related to two other dots. This relationship would still be seen with the dot occupying any one of an infinite number of positions on the line terminated by the other two dots. It is not necessary to make quantitative measurements to be able to make the judgment of between-ness. If more dots be added to the line of three, more complex relationships between the dots can be built up by using this same judgment of between-ness. This appears to be one way of relating successively the idea of an order or a sequence, to elementary human judgments in which observers reach agreement.

A few examples showing order may serve to clarify the use of the term: A list of Presidents of the Royal Society arranged in the order of their first tenure of office would be a set of things arranged in order or sequence. The same members of this series could be arranged in many other ways, for example, according to height or weight or alphabetically according to name. In considering the order, interest would be confined to the position only of one member with respect to other members.

When one arrangement is preferred or is very familiar, or very apparent, or for any reason is assessed high in value, it is often referred to as *the* order. For example the order of integers refers to their arrangement in order of magnitude, although there are many other ways of arranging them in a series or sequence. The number of ways of arranging n things in a series—the number of their permutations—is factorial n or $n! = n(n-1)\ (n-2) \ldots 4, 3, 2, 1$. All these permutations are not, however, of equal interest to all men at all times.

If a series of numbers be written down thus: 2, 4, 6, 8, 10, 12, 14, 15, some kind of order would be readily seen to hold as far as 14, but to be spoilt by the 15. By extending the

series of numbers beyond 15, that is by putting the whole series 2 to 15 in a different environment, order could be seen again. In the simple example 1, 2, 3, 5, 6, 7, 9, 10, 11, 13, 14, 15, etc., an order or sequence can be seen which we should fail to see in such selected portions as 2, 3, 5, or 7, 9, 10, or 1, 2, 3, 5, 6. Moreover, if we saw the beginning 1, 2, 3, and jumped to the conclusion that the next number was 4 we should be in error. A consideration of this very simple example of an arrangement of numbers illustrates several facts, about order and sequence, which may help in the understanding of those selections of facts in which so far no order has been seen.

Firstly, some of the properties of a number, object, or fact are lost by removing it from the environment in which it was found before starting to study it. The number 2 loses some of the meaning it has for us when it is removed from the collection of numbers just given. Secondly, the same is even true of groups of numbers or in general of groups of things removed from one environment and put in another. The attempt to guess some of the properties of a whole from properties of selected parts may be either successful or unsuccessful. If it is successful, well and good, but if it is unsuccessful there are no rational grounds for surprise. Neither the sequences 1, 2, 3, nor 2, 3, 5, nor 3, 5, 6, can alone help us to see the order apparent in the whole series from which they are selected.

The device of guessing that some order or sequence found in experiments will be found beyond the range of the original experiments is called *extrapolation*. Agreement between the guess or extrapolation and the results of later experiments in the extended region is not always found, and the sceptical scientist leaves extrapolation to others. In other words a selected sample of facts may show an order different from that shown by a whole region. Adding a new fact to knowledge may spoil an order already perceived, as did the addition of the number 15 to the series of numbers first given. A total eclipse of the sun upsets a very simple and regular order or sequence of day and night which may be observed hundreds of times before it is upset by the solitary

eclipse. Still more facts have to be taken into account to see order again in the whole sequence of normal days and nights and the abnormal darkness or night of the total eclipse. On the other hand addition of one or more facts may introduce order into a large collection of facts.

This is only another way of saying that order and sequence are pattern properties or properties of wholes. The addition of one or more facts to a group alters the group or pattern treated as a whole. The mere addition of one line AB to Fig. 15 of page 117 introduces some quite new properties to the whole, which could not be seen as inherent in the figure without the extra line. On the other hand quite a large number of lines could be added without giving the particular kind of order seen in Fig. 12. Order may be brought to fields of knowledge, which at present seem chaotic, by the addition of only one more fact. On the other hand the addition of many facts may still fail to reveal any order.

Order or sequence in sense data obtained by ear seems to depend upon the elementary judgment of one sound following or preceding another. This, like the visual judgment of between-ness, is one in which agreement between observers can be reached and is the basis of the judgment of coincidence of two sounds. Some typical examples of order in sounds is observable in the ticking of a clock or in the rhythmic drum beats used in music such as the Ravel *Bolero*.

One kind of order often found is in the regular repetition of some unit which itself may show no order or sequence. Examples are night and day, the seasons and the tides, bodily metabolism, and other periodic chemical changes, vibrations of all kinds, and the space lattice of a triclinic crystal. In the individual unit no order can be seen, or alternatively the order seen differs from that shown by the repetition of the unit.

COMPLEX ORDER

Before applying the idea of order to explain the scientific law and scientific classifications, a more complex example

of order will be considered. This more complex example may be used as an analogy to help to an understanding of the great difficulty in co-ordinating or seeing order between the subject matter of the different sciences.

If we first think of a mathematical series and compare it with something visual we may regard it as a series lying upon a straight line, as something showing order and extending in one dimension. Our use of a visual or geometrical analogy suggest our thinking of more than one dimension. We might write underneath one mathematical series several others, each of which would show a different order considered by itself. If then this collection of different mathematical series were regarded as a whole we should have something showing a much more complex order than that of any one mathematical series. Using letters for simplicity of writing we might have:

A	B	C	D	E	etc.
F	G	H	I	J	etc.
K	L	M	N	O	etc.
P	Q	R	S	T	etc.

to represent only four simple series written along horizontal straight lines. But order might then be seen in such series as C, H, M, R, etc., lying on the vertical lines and in such series as A, G, M, S, etc., or F, M, T, etc., lying on sloping lines. The whole group of series could be visually extended by grouping the different series in three dimensions and it could be mentally extended into *n* dimensions. If attention now be concentrated upon one symbol, say M, then it is evident that the whole group of series contains many individual series, each containing the term M, and each showing a different order. This gives us a way of thinking of scientific co-ordination. Each separate series would represent some relationship or order perceived to hold between the thing represented by M, and the other members of the group. For example, if M represented the element iron, then one series might represent the order or relationship between the magnetic properties of iron and that of other elements;

and another series might include biological properties as when iron occurs in haemoglobin, copper in haemocyanin, and magnesium in chlorophyll. The whole group of lines passing through M would co-ordinate all the known scientific facts about iron.

The whole idea of this section is difficult to express. I do not know if this type of grouping of mathematical series has been elaborately developed. It is evidently one of considerable difficulty, for it clearly deals with complex pattern properties and we have already seen that the properties of a whole or pattern may be considerably altered by the addition of merely one thing to it. To group together either mathematical or any other series having order, in such a way that certain other orders shall be seen also in the complex whole is evidently, by analogy, a task of extreme difficulty. In scientific research we are able to work with any one series as, for example, when the magnetic properties of all the elements are studied, but when we turn to a series showing a different ordering, as in studying some other property of the elements, it is often difficult to see a more complex order which shall include both the series.

Remembering that scientific data are always data selected from a vast number of possibilities, it is evident that a re-selection may give an ordered series fitting into a complex group of other series. This is perhaps only another way of saying that a new point of view may enable us to see order in the facts of two different branches of a science. Research workers vary in their interest in seeking, and ability in finding new relationships, although most are delighted when two fields of study can be seen as related. Faraday discovered many new facts, but to the end of his life he never lost interest in his attempts to find relationships between different types of phenomena. His last experiments, dated March 12, 1862, were concerned with his attempts to co-ordinate optical and magnetic phenomena. As an example of a number of series of data, each series showing order, which cannot at present be fitted into a complex whole showing order or relationship the current research work on cancer may be quoted. A number of different 'lines' of research

all give results showing definite order or relationship, but at present they cannot be fitted together into one ordered whole.

When order cannot be seen in a collection of selected facts, the facts can sometimes be grouped together by fitting them into a Scientific Theory. This method is discussed in later chapters.

THE SCIENTIFIC OR NATURAL LAW

The formulation of a scientific law or a law of nature seems to depend upon the perception of order in a number of abstractions; in practice it is also combined with the idea discussed in Chapter III as the Uniformity of Nature, for a law of nature is usually regarded as telling something useful for future action, as well as something about order seen in the past. Beginning with observation and continuing with abstraction, order is noticed in some collections of abstractions. After a time more and more concrete objects or events seem to be noticed from which the same abstraction can be made. With multiplication of examples we become more venturesome and begin to predict what will be observed in circumstances which seem similar to those already observed. If the predictions fit the facts established by subsequent observation, a generalization is made leading to the statement of a scientific or natural law.

Galileo, examining only a few concrete examples of falling bodies, abstracted certain results and generalized them into laws of motion of falling bodies. All such generalizations are a matter of past experience. It is found in practice that some men on some rare occasions can select from a vast diversity of concrete objects or events certain common similarities. They can see order amongst much disorder, and can make such selection that they can predict what will be observed in the future. When their predictions are fulfilled, they are said to have discovered, or stated, a law of nature. No such prediction can be made with certainty, but it is found in practice that many such predictions have so far been invariably fulfilled, and such are called established natural or scientific laws. There is, however, no absolute

guarantee that they will in the future continue to fit the facts. We should be said to be acting rationally if we used the idea that they would, and irrationally if we behaved as if they did not, continue to do so. But scientific laws are not absolute.

They are generalizations of some perceived order, and can be used in future action as if that which has been observed to happen in a restricted number of concrete instances will continue to be observed in similar conditions. Their precise formulation often necessitates the use of technical language. For example, suppose it be said that Galileo's experiments carried out at Pisa established the natural law that light and heavy bodies released at the same time fall through the same distance in the same interval of time. The non-scientific reader may well ask if Galileo tried dropping a feather and an iron weight simultaneously from the tower. Clearly such a statement of the law does not fit all the facts. This type of difficulty necessitates the use of technical terms, if concise statements of scientific laws are demanded. The discussion of such apparently anomalous examples will be deferred until the technique of experiment is dealt with (Chapter XV).

The scientific law is based upon observation and abstraction, and on the perceiving of order in the results of these. But order is a property of a whole or a pattern. What is the whole concerned in a scientific or natural law? It seems to be neither the external environment alone, nor the human observers alone, but must be considered as a property of the two considered as the whole. By this I mean to imply very definitely indeed that a law of nature is not something quite independent of human beings. A natural law is an order perceived by a human observer in his abstractions from his environment, that is, from the parts of nature which he has been studying. It is neither something invented by man, nor is it a mechanism of nature. It is kind, but not logically essential to suppose that nature is a going concern which has to be run by the scientific laws of nature in much the same way as a state has to be run by human laws to produce certain results.

To make this point clearer let us think of some definite concrete example. Suppose that I am meditating in a boat on a river on a calm day and note water occasionally dripping from an oar into the unruffled surface of the river. What happens is said to be governed by natural laws. The motion of falling drops and of the surface of the water and of the air carrying the sounds of the dripping could all be represented by differential equations which could be solved by one method or another. Numerical values could then be substituted for the symbols and could be used to give definite information about what could be observed after, for example, a drop of water left the oar. It does not, however, follow that because a physicist can predict with the aid of a differential equation what will happen to the drop in falling, that therefore the drop, in falling, is in effect solving a differential equation or that it must, so to speak, know about the equation in order to move as it is observed to move. It is only necessary to recollect that man can sometimes make similar predictions about the same phenomenon by alternative methods. If it is supposed that the natural laws formulated by man are devices used also by concrete objects and events, then it must be supposed also that sometimes a falling body is using an analytical, and sometimes a graphical, method, to determine its path. Kepler's Laws of Planetary Motion are not to be regarded as laws which planets obey, but as an order which man can see in observing the motions of certain planets.

To avoid confusion of thought it is essential to remember that no scientific law ever deals with concrete things, but only with abstractions from them. Newton's Law of Gravitation does not deal, amongst other things, with falling apples, tides, and moving planets. It deals with some particular aspects only of them. There are hundreds of things which human observers can notice about apples, tides, and planets, about which the law of gravitation has nothing whatever to say. We cannot therefore look at some particular object or event and say that is an example of such and such law of nature. We can say only that if hundreds of things seen in the object or event be ignored, and attention

be concentrated upon a few details, then those details abstracted from the whole fit such and such a natural law. If this distinction is not understood then it may lead to the false idea that some particular object or event can violate a law of nature.

In concise statements of laws of nature their range is often not indicated. This omission then leads to such a statement as that some phenomenon violates a law of nature. Such a statement—a contradiction in terms—appears to be based sometimes upon a confusion of a hypothesis or theory with a scientific law.

A scientific or natural law is a statement of some order seen in observing objects or events. Since order is a property of a whole or pattern, it follows that a law of nature refers to some whole, to some collection of facts. It is evident that additions to this collection of facts may or may not upset the order previously seen, and it may or may not introduce a new order which when stated would be a law of nature. Any such statement refers only to the facts examined at the time of formulation of the law. If a scientific law is confused with a scientific theory it may be expected to tell something about objects or events outside the range of those first considered when the law was formulated. A law is not so much a tool of research as a concise statement representing a vast number of concrete examples. It helps by economizing labour but cannot act as a pioneering tool.

A natural law summarizes many facts and these same facts may later be fitted into a theory. The formulation of a scientific law is a very great help to future research workers or to others who may wish to use the facts covered by it, but it is not a device which can invariably be used to deal with facts lying outside the range of those considered when the law was formulated. If some property of a number of gases is studied at temperatures lying between 0° C. and 100° C., and if some order can be seen in the results, then a natural law can be formulated. If later the same property of these gases is studied at temperatures lying between 100° C. and 1,000° C., and if the new results do not show the same order as the old it is unscientific to say that the natural

law has broken down at high temperatures. A scientific law about some property of gases at high temperatures can be formulated *only* on the basis of observations carried out with the gases at high temperatures. The formulation of a natural law follows, not precedes, the observations. The devices of the hypothesis and the theory which precede further observations are not the same as the natural law.

Another example of what is by some called a violation of a law of nature would be seen when a copper ring lying at rest suddenly jumped upwards to a height of several feet without any motion of the table on which it was resting. The experiment is one which is often shown at popular science exhibitions and depends upon the use of electrical devices. Here natural laws of mechanics and also of electricity must be used to express order which can be seen in what happens. Many other things which can be observed at the same time are not covered even by taking into account both mechanics and electricity. Some data which can be selected from observing a phenomenon are dealt with by one law, some by another, and some by no law at present known. The selected data either fit into known laws or they do not. Those which fit into one law do not violate any other laws (which are irrelevant to the consideration of the selected data). Furthermore natural laws do not represent *tendencies* of things to be or do certain things. Where they fit the facts at all, they fit them precisely. They are not made to fit concrete objects and events, and they never do so. They fit precisely those abstractions or selected details which they are made to fit.

It will be evident that the treatment here given of the scientific law depends essentially upon the pattern idea discussed in Chapter VII, for order is regarded as a special case or perhaps as one of the elements upon which some of the properties of wholes depend. As I see it, this view seems to fit the facts better, and, if the whole view is accepted, there are no rational grounds for surprise when new facts either do not or do fit into existing natural laws formulated before their discovery; for sometimes the properties of wholes are considerably modified by the addition of

fresh things to them, and sometimes they are but little altered.

Before leaving the subject of scientific law a brief reference will be made to the occasional confusions, by the non-scientific writer, of a human with a scientific law—for example, Professor Plate's statement, "I always maintain that if there are laws of nature, it is only logical to admit that there is a law-giver."* From what has already been said it will be evident that I do not regard a law of nature as a mechanism by which an observed effect is produced. I do not agree with the serious personification of a Nature who rules phenomena by the so-called laws of nature in the same way that states are governed by the laws of man. A human law is a statement of what a man or men must do to avoid the risk of imprisonment or fines. All human laws are about what men can, but may or may not, do. Human laws provide for some men doing, and some not doing, a thing. It will be evident that, apart from the common use of the word law, no simple relationship can be seen between a human and a scientific law, for the latter is no more than a statement of an order seen by man in a collection of abstracted data.

CLASSIFICATION

So far in this chapter attention has been directed to methods used in scientific research for dealing with the concrete objects of the laboratory and of observation by selecting some particular detail and concentrating attention upon the abstracted data. In the scientific law the scientist expresses his recognition of order in the selected data. Throughout, the perception of similarities has been important. This same recognition of similarities is the basis also of classification, in which things having some similarity are thought of as a single group, as one class, instead of as a number of separate things.

Classification might be defined as the process of recognizing at least one point of similarity possessed in

* F. Wasmann, *Problems of Evolution*, p. 108. London, 1909.

common by a number of individuals, and of mentally associating those individuals so that they form one group or class. In practice each of the individuals is sufficiently different to be recognized as an individual, that is, each individual has sufficient dissimilarity to be quite distinct from other members of the class. Classification would be in use if a group of individuals consisting of steel, golf, cricket, foot-, tennis, billiard, croquet, and ping-pong balls were mentally associated and described as, say, spherical objects.

To avoid mental confusion classification is necessary to man because of the immense variety of objects and facts which come into his life and because of the inability of even the greatest minds to deal with a large number of ideas or impressions individually. The mind seems incapable of dealing with many things at once. The active mind seems able to jump rapidly from the contemplation of one thing to another and to be able to store many facts and ideas so that they are readily available for use, but no human mind seems capable of absorbing and using the whole of human knowledge in all its details. Instead of attempting this apparently impossible task, men seek to group the numerous individual things and facts of human experience into classes, the members of each class having as many points of similarity as possible. It is then found that for many purposes of thought the one group or class may be substituted for the many individuals of the class, to reach results which may well be observed for all the individual members of the class. The important point to notice is that by finding some way of reducing a number of facts or ideas to one thing, a class, something usable is obtained. The mental confusion inevitable in an attempt to deal simultaneously with many things is avoided. Once some result has been obtained the process can be repeated with each of the individuals of the group or class. If the result obtained by ignoring details still holds when the details are examined carefully, then the classification has enabled us to carry out a process of thought which would otherwise have been impossible. A process similar to that of classification is in use in doing a piece of applied mathematics when we write

"Let x be the distance of the moving particle from the point O." If x is a variable, we know that it may have perhaps an infinite number of values lying between, say, 90 cm. to the left and 90 cm. of the right of O. Yet by recognizing that in all the diversity of positions occupied by the particle at different instants, there is sufficient similarity to enable us to choose one position, at x cm. from O at the instant t, much detail is reduced to a simple thing with which the reasoning process can then be completed.

So far classification has been spoken of as a necessary tool of thought—a means by which action of the mind is possible in the presence of much detail. On this view classification seems to be associated with the making of mental concepts. The single thing called a class used in reasoning is probably a purely imaginary entity. In thinking of a 'metal' as such, we do not perhaps think of any actual metal such as copper, iron, or aluminium, but of some imaginary substance which has all the properties given later in this chapter as characteristic of metals. If the mind begins to think of copper it may be distracted by thinking of electrical conductivity, if of iron, magnetism may intrude, if of silver, photography may come into consciousness. Classification then helps us to substitute imaginary things, abstract ideas for sense impressions of concrete things.

When objects or events have been grouped together into classes the process can sometimes be repeated. Similarities can be seen amongst some of the classes, and these can be mentally associated into one larger class and so on. The process is much used in the sciences dealing with living things of all kinds. In some circumstances order can be seen in a number of classes and this perceived order may be used to group classes together and may lead to such an arrangement as the periodic classification of the chemical elements. The term classification is used for arrangements of either kind, but in scientific works it is not applied so much to the simple grouping of a few things into one class. The device is in use when any similarity is recognized in even two objects or events, whether the process be called classification or not.

Classification is used as much in everyday life as in scientific research. For example, on some occasions things as diverse as beef, mutton, lamb, veal, and pork are grouped together and called meat. Again, sole, salmon, turbot, and halibut may be thought of as one class, fish. On other occasions these two classes can be grouped together into one class, food. The study of language shows that men have used classification from the earliest time of which records are available, and all processes of reasoning involve use of the device. The classification of the chemical elements is an excellent example of the use of classification in scientific research and will serve to illustrate some of the factors so far discussed.

CLASSIFICATION OF THE CHEMICAL ELEMENTS

The classification of the chemical elements is a problem to which a great deal of attention has been given. Some of the best-known classifications will serve to illustrate the technique of classification.

One of the simplest was the placing of each of the elements into one of two classes, metal or non-metal, by Berzelius at the beginning of the nineteenth century. The classification is one in which the members of one class tend to have many points of similarity. The general tendencies shared by all the metals are that they form basic oxides, they dissolve in mineral acids giving off hydrogen, they do not readily form stable compounds with hydrogen and in the state of vapour their molecules are usually monatomic. Their characteristic physical properties are high density, metallic lustre, malleability, and ductility, they are usually solid at ordinary temperatures and volatalize only at high temperatures, they are good conductors of heat and of electricity, and their electrical resistance usually increases with rise of temperature. There is here a grouping together of individuals which have not merely one but a dozen or so of points of similarity in qualities studied in chemistry and physics.

The characteristic non-metals form acidic oxides and

stable compounds with hydrogen, they do not readily dissolve in mineral acids, many are gases or liquids at ordinary temperatures or can be readily volatalized, and in the state of vapour their molecules are usually polyatomic. In bulk they do not reflect light very well, they are bad conductors of heat and electricity, and the electrical resistance usually decreases with rise of temperature. Their density is low and malleability and ductility are not well defined. Here again is a group of distinct individuals with a dozen or so points of similarity. Not only are these points of similarity in the class, but they are points of dissimilarity with the members of the other class, the characteristic metals. However, the chemical elements cannot all be put into one of these two classes. Some of the elements such as arsenic, antimony, and tellurium have some of the main characteristics of metals such as metallic lustre, high density, and high thermal and electrical conductivity as well as some of the characteristics of non-metals. The non-metals carbon, silicon, and boron are less volatile than most metals. The metals sodium and potassium are so light that they float on water. The non-metal graphite is a good conductor of electricity. If any one of the characteristics of either class be chosen it may well be possible to arrange the elements in an order in which there is a gradual change from non-metal to metal.

This latter feature that order can often be seen in some quality of the members of one class is very common in classifications. It can be readily illustrated where the particular order considered is an order of magnitude and it will be discussed later.

Other classifications of the chemical elements are into acidic and basic or into electro-negative and electro-positive whilst in analytical chemistry they are classified according to their reactions with certain reagents which enable them to be separated into five groups. The most comprehensive classification is in terms of a property which does not change, during chemical reactions, the so-called atomic weight. Much hypothesis and theory preceded the idea of atomic weight, but when the idea was developed chemists began

to notice order in atomic weights of different groups of elements. Döbereiner in 1829 published one of the early papers on the subject. He noticed that elements with similar chemical properties had almost the same atomic weight or else the atomic weight of the elements in one group (triad) differed by an almost constant amount. Newlands in about 1865 arranged the elements in the order of ascending magnitude of their atomic numbers and noticed that each eighth element was a kind of repetition of the preceding eighth.

Independently Mendeléeff in 1869 studied the subject and stated, "When I arranged the elements according to the magnitude of their atomic weights, beginning with the smallest, it became evident that there exists a kind of periodicity in their properties. . . . I designate by the name 'periodic law' the mutual relations between the properties of the elements and their atomic weights. These relations are applicable to all the elements, and have the nature of a periodic function." It will be noted that this is an example of classification arising out of perception of order. From the observed periodicity the elements are classified into groups with similar properties which gradually change from member to member of the group. But the members of one group resemble each other in chemical properties more than do any of the other elements.

The classification is one in which when the classified members were put into classes because of a perceived order it was found that members of one class had many points of similarity. This quality is one which always pleases the scientist, the more similarities found in the members of one class the better the classification, as such, is liked. Many similarities are seen not only between abstracted qualities of the elements, but also between their compounds. If the members of one class be combined with the same element or elements, the resulting compounds are similar in many respects. In the classification as a whole, order can be seen in the following abstracted qualities of the elements: Specific gravity, atomic volume, melting point, hardness, malleability, ductility, compressibility, coefficient of expan-

sion, thermal conductivity, latent heat of fusion, refractive equivalents for light, colour, and electrical conductivity.

Even at the time of Mendeléeff's work the classification was remarkable for the amount and variety of the data incorporated. Order plays a large part in the scheme and the discontinuities seen at the time stimulated much research, illustrating that the perception of order can be a tool of research. It was supposed that some gaps in the order seen were due to lack of knowledge and that some elements which had never been observed nevertheless could be found. One example will be given. In 1871 Mendeléeff predicted that an element which he called Eka-silicon (Es) and which was unknown at the time would have certain properties an account of which he gave in detail. In 1886 an element was discovered and called Germanium (Ge) and this element had properties very similar to those given by Mendeléeff to the imaginary substance Eka-silicon. The closeness of fit of prediction and observed fact may be judged from the following list. The data given in brackets are those of germanium:

Eka-silicon, Es, will have atomic weight 72 (72·3), specific gravity 5·5 (5·47), and atomic volume 13 (13·2). The elements will be dirty grey (greyish-white), and on calcination will give a white powder consisting of EsO_2 (GeO_2 is white). The element will decompose steam with difficulty (Ge does not decompose water). Acids will have slight action and alkalies no pronounced action on it (Ge is not attacked by hydrochloric acid or KOH solution, but reacts with aqua regia and with fused KOH). The element will be got by the action of sodium on EsO_2 or on EsK_2F_6 (Ge can be got by reducing GeK_2F_6 with sodium). The oxide EsO_2 will be refractory and have specific gravity of 4·7 (4·703). The chloride $EsCl_4$ will be a liquid with a boiling point under 100° (86°) and a specific gravity of 1·9 at 0° (1·887 at 18°). Eka-silicon will form an organo-metallic compound $Es(C_2H_5)_4$ boiling at 160° (160°) and with specific gravity of 0·96 ($Ge(C_2H_5)_4$ is slightly less dense than water).

Equally successful were Mendeléeff's predictions of the

properties of eka-aluminium (gallium) and of eka-boron (scandium). The discovery of the inert gases argon and helium had not been foretold from Mendeléeff's classification, but when it was altered to include these two elements, gases with the properties later found in krypton, neon, and xenon were sought.

The classification also helped to fix the atomic weights of certain elements for which the current methods failed because of experimental difficulties. For example, that of indium was changed from 75–6 to 113–14, and beryllium, uranium, and some of the rare earths were given values which fitted the results of later experiments. The perception of a gradual smooth change in the values of each of the constants of the elements in each of the groups or classes led Mendeléeff to predict that the atomic weight of platinum, iridium, and osmium was too high. More careful work has confirmed this prediction based upon the classification and can be seen in the following list of atomic weights:

	Platinum	*Iridium*	*Osmium*
In 1870	196·7	196·7	198·6
In 1919	195·2	193·1	190·9

Although the periodic classification of the chemical elements includes a great many data it leaves out of account a still greater number. To understand this it must again be remembered that the data of scientific research are selections made by human beings from a vast number of selections possible to man. In the stores of a chemical laboratory the bottles of chemicals are usually arranged on the shelves according to the periodic classification of the elements. In the Science Museum at South Kensington, an exhibit of samples of all or most of the elements arranged also according to this classification can be seen. When looking at such concrete objects, we are apt to forget that the periodic classification of the elements deals only with certain selected properties of all specimens of, say, iron. The observer of such an exhibit is very vitally concerned with one very remarkable property of iron, that it can form part of the compound haemoglobin in his blood. The periodic

classification tells nothing about this, or about the fact that the other elements in the same class, in spite of many similar properties, can apparently not replace iron in this remarkable substance. The periodic classification, like all other scientific classifications, deals with similarities in certain selected qualities of objects and events, not with objects or events each regarded as a whole. Classification is a mental process. The placing of bottles of chemicals in a certain order on a shelf may be based upon a classification, but such order as is seen, is order in certain selected qualities of the substances in the bottles, and not order seen in all of their known qualities. This may lead us to the consideration of the so-called natural and artificial classifications.

NATURAL AND ARTIFICIAL CLASSIFICATION

When men apply assessment of value to classification there is the same lack of agreement between different assessors as is found wherever this unscientific device is used. Some classifications are called 'merely' artificial or arbitrary, and are sometimes assessed of low value, others are said to be natural or scientific and are assessed as of high value. It is difficult to apply pure reason to these evaluations. If 'artificial' means using some artifice or device, then all classifications are artificial. If artificial means not natural, then some clear idea must be got of what is meant by natural. If natural means occurring in nature in the external world, then the periodic classification is not natural, because the elements are not all found in nature separate from other elements, and because those which are found outside the laboratory are not found arranged in the classes of the periodic classification of the elements. 'Nature' is sometimes taken to mean 'things outside of the human observer'; this discussion is not, however, concerned with arrangements of concrete objects, but with arrangement of abstracted qualities, that is, with mental concepts. These are to be found only in men's minds, and would therefore be called unnatural by those who do not regard man as a part of the nature studied in the non-biological sciences.

Those classifications of the material of the exact sciences which are often called good scientific classifications, appear to have one feature in common; the members of each class have many points of similarity in features which are studied in the particular science concerned. The members of a class may be put into that class because of similarity in some one respect, and it is then found that they are similar in other respects. In those classifications assessed as of low value, or as artificial, the members of a class have few of such points of similarity. A classification of the elements into classes according to the initial letter of the name is artificial and places in the same class such elements as aluminium, argon, and arsenic, having few points of chemical similarity. The classification according to atomic weight or number is called natural or 'good,' and it places in the same class elements having many points of chemical and physical similarity.

It will be noted that the chemist and physicist as such is not necessarily interested in the biological function of the elements, and the periodic classification does not, I think, reveal any perception of similarity of elements which have important biological functions, or of difference from those elements which have no such function. It is known that life is never found separated from the type of chemical compound called protein, and it is known that certain of the elements are always found in proteins. But in so far as I am aware, the periodic classification tells nothing about those properties of the elements always found in protein which helps us to see any relationship between them, or any common difference between them and those elements never found in protein. Neither does it help us to see why nickel cannot be substituted for iron in haemoglobin to give a compound of similar biological function. Many examples have, however, already been considered of wholes having properties which cannot be seen as inherent in the separate parts into which they can be divided, mentally or physically. Although the periodic classification is an example of a natural classification, it is evident that it does not include all of those selected qualities of elements which may have human interest; in terms of pure reason

it is difficult to see why one kind of classification which deals with only one part of nature is called natural and not artificial.

Some modern writers on scientific method still seem to see no objections to the use of the terms natural and artificial. Thus Wolf states, "The aim of scientific classification is to see things according to their actual objective relationship. Such a classification is what is meant by a *natural* classification. . . . Classifications made for special, practical purposes are usually called *artificial* classifications. They are not made from the standpoint of the objects themselves, so to say, but from the standpoint of the practical needs of man."* This view, presumably based upon the Absolute theory of science, differs from that adopted in this book in appearing to exclude the classifier from the classification. Such phrases as: 'Actual objective relationships' and 'standpoint of the objects themselves' seem to suggest that scientific classifications are not man-made, but are rather something 'actually' occurring in collections of concrete objects. As I see it, Bain's classification rule seems to fit the practice better. He wrote, "Place together in classes the things that possess in common the greatest number of attributes."† Because of the fame in physics of Ampère, his views on classification will be given. They are typical of much writing on classification. "We can distinguish two kinds of classifications, the natural and the artificial. In the latter kind, some characters, arbitrarily chosen, serve to determine the place of each object; we abstract all other characters and the objects are thus found to be brought near to or to be separated from each other, often in the most bizarre manner. In natural systems of classification, on the contrary, we employ concurrently all the characters essential to the objects with which we are occupied, discussing the importance of each of them; and the results of this labour are not adopted unless the objects which present the closest analogy are brought most near together, and the groups of the several orders which are formed from them are also

* *Essentials of Scientific Method*, pp. 31–3. London, 1925.

† *Inductive Logic*, p. 185. 1870.

approximated in proportion as they offer more similar characters. In this way it arises that there is always a kind of connection, more or less marked, between each group and the group which follows it."* Jevons is dissatisfied with this statement and asks in effect, what tests are to be applied to judge importance and closeness of analogy. He makes "no sharp and precise distinction between natural and artificial systems . . . a natural is distinguished from an arbitrary or artificial system only in degree. It will be found almost impossible to arrange objects according to any one circumstance without finding that some correlation of other circumstances is thus made apparent."†

A classification according to names arranged in alphabetical order is often used as typically arbitrary, but such an arrangement of men's names may place many Evans's and Jones's under E and J and many Mac's under M. A name beginning with the same letter would not be the only point of similarity, for the Evans's and Jones's would be Welsh and the Mac's Gaelic, and the speech of the Mac's might show a common similarity different from that of the Jones's and Evans's. Although many factors are involved in the naming of objects I see no rational grounds for supposing that no order can ever be seen in the signs and sounds used as symbols for objects and ideas. In this connection it is only necessary to remember pictorial writing, onomatopoeic words such as 'Cuckoo,' and the facts used to illustrate the gesture theory of speech where, for example, the speaking of a word used to denote digging necessitates a scooping movement of the tongue.

This discussion of classification may be summarized as follows. Classification is a mental process in which the mental concepts of two or more objects each regarded as a whole are grouped together and regarded as forming one class. The factor which determines this association is recognition, by the classifier, of similarity in some selected quality or qualities of each of the objects or ideas dealt with in the classification. When assessment of value is applied to

* *Essai sur la Philosophie des Sciences*, p. 9. 1838–43.

† *Principles of Science*, vol. ii, p. 352. 1874.

a classification, it is rated as of high value or is called scientific or natural or good, if the members of each class have many points of common similarity of interest to the classifier. If the members of the classes have few points of such common similarity of interest to the classifier, the classification is called artificial, or arbitrary, or unscientific, or unnatural, or poor, or of low value. If the assessor of of value is not concerned with absolute values, then he may say that an artificial classification, such as the alphabetical index used in all scientific journals, is a good classification and he may display signs of annoyance if he is unable to find such an arbitrary classification of the contents of some scientific journal in which he is interested. Those classifications such as the periodic classification of the chemical elements, in which order can be seen in the collection of classes, are generally rated especially high in value. At least one human being, the classifier, is an essential part of a classification, for classification is based upon perception of similarity by a human being. Since judgment of similarity, unlike judgment of coincidence, is not a judgment in which anything approaching universal agreement has so far been reached by men, it is not surprising that no universal agreement has been reached in classifications. New knowledge of selected qualities of the same concrete objects often suggests new classification.

CHAPTER XI

PATTERN IN ACTION

On the patterning theory of science, classifications, laws, and theories are regarded as ways of arranging facts. They are biological products of scientists, each of whom has his own unique internal (biological) and external environment.

Must those scientific actions which give arrangements of facts, be regarded as unrelated to other human action? In this controversial chapter an attempt will be made to develop an idea serviceable in analysing all observable action. Attention will be confined to such human action as can be observed by an external observer (using coincidence observation), but no other restriction will be used in the choice of examples. As a scientific outlook is to be used, no attempt will be made to say *why* any particular action is carried out or how the action will appear to the man doing it.

ONE CHARACTERISTIC OF NORMAL HUMAN ACTION

A suitable starting-point is the idea of aim or purpose which has already proved very useful in analysing many actions. Conventionally it is often considered satisfactory to analyse a man's action in terms of his aim. The statement that in cricket a batsman's purpose is to make as many runs in as little time as possible, is readily understood. But is it satisfactory on second thoughts? How does it come about that in practice the batsman does not go to the cricket field and do his running when he can have the field to himself? Ultimately it would be necessary to explain the purpose of all the other men in the field. In practice we are well aware of all these other factors, and add a great deal more than is expressed in the simple statement of the batsman's aim.

The idea of purpose can be included in the more general idea of pattern as described in Chapter VII. It may then

be stated as a hypothesis that *one* of the common characteristics of normal human action is that each action fits into, completes, or makes a pattern or whole. In practice we are not always indifferent as to how the aim is reached. The whole of the details of the action seems to matter, and the hypothesis is devised to try to include this fact. From the very nature of the pattern idea, the pattern of any individual action cannot be symbolized by words or anything other than the complete action. Purpose and aim are to be regarded as related to the pattern of an action, as a rough outline sketch is to a landscape.

So many ideas have been used in explaining action and adult man is so highly skilled in inventing rationalizations that it is difficult to test the usefulness of any alternative hypothesis. Social conventions often compel us to invent the most wildly fantastic 'reasons' for doing an action. Because of these social conventions we are apt to pay no attention to explanations which make no pretence whatever of giving 'reasons' for an individual doing an action. It is evident that the patterning hypothesis fits many scientific actions, including logical argument, but is it invented to fit only these scientific activities? Are human beings ever seen behaving so that they seem to be fitting actions into patterns, *without* regard for any subsequent meaning the pattern may have for them? To attempt an answer to this question actions must be sought for which there is the greatest difficulty in inventing rationalizations.

At first sight the facts to be discussed seem to have little relationship to scientific activities, but the reader is asked to regard this chapter as a controversial interlude in which an attempt is made to see a relationship between some scientific activities and human action in general. It is an attempt to make the Patterning theory of science seem less 'arbitrary' than appears at first sight.

THE PATTERN IDEA IN CHILDREN'S ACTIONS

It is evident that consciousness of the pattern of an action is not essential in the doer, for the observable breathing

and heart-beating fit into patterns of which the individual may be unconscious. The extreme regularity seen in these actions is as rare in general, as is symmetry in visual patterns. More complex and irregular actions can be seen in the child's nursery. Let us go to the cupboard and hunt amongst the litter for some of the child's first books, some of the books it had before it could read. What did it do with those books when it was left to itself with pencil or crayon? It began to see the patterns which adults call letter *o* and it also saw other things like *c*, *e*. Did it like the *o* pattern better? At any rate it had carefully completed *c*'s and *e*'s into *o*'s. Then perhaps it has started filling in with pencil all the *o*'s and those letters which have *o*'s in them, a, d, g, p, and so on. Has the child been completing or filling in patterns? When I see a page on which this mutilation has been carried out on only a part of the page, I find myself thinking, 'If you must mess the page up in that way, why not complete the work?' I find myself at once, but almost unconsciously, wanting to see the pattern of filled-in rings completed over the whole page. In other books, especially those with large letters, it may be found that the child has not converted a *c* into an *o*, but has drawn a continuous line all round the *c* both inside and outside, the printed letter being completely enclosed in a double *c* outline. This pattern of outlining letters is carried out instead of filling in *o*'s. With the very large type letters of posters, children often trace with chalk over each line of the letters. Incidentally it may be noted that not only do young children seem to regard print as pattern, but poster artists and printers often seem to ignore the usual meaning of print, and the advertisement designer sometimes arranges the letters so that although they make a pattern it is very difficult to read the words. The printers of such a paper as the *Radio Times* often set out the time tables of programmes to be broadcast in such a way as to make an attractive pattern of type upon the page rather than to convey rational meaning as simply as possible.

A second example is in children's painting books. In these, line outlines are given and children seem thoroughly

to enjoy filling in the outlines with crayon or paints. It is important to note that there is a tendency not to bother about filling in the outlines as in the coloured illustration often given on the opposite page. It seems to be enough to fill in the spaces with different colours. There seems to be no simple relationship between the activity and the representing of objects which the child has seen. Even a child not colour blind, if left to itself with some outline pictures, may paint hair in the picture bright blue or green. So long as the space is filled with some colour different from its neighbours the particular colour does not seem to matter. All the spaces must not be filled with the same colour for then there would be no pattern. This example seems to illustrate the love of pattern and the love of putting things into patterns or rather it seems to show children fitting things into patterns and not paying much attention to any rational meaning of the pattern.

For the third illustration it is necessary to have the child in the nursery. One of the standard toys is a box of building bricks. When the child is first given the toy it is shown the illustrations of houses which can be built with the bricks, and probably with adult assistance it copies the models. Later on when the novelty of the toy has worn off the child starts to do different things with it. When left alone it may build the bricks up into 'meaningless' structures. It may put two this way and two that, or alternately three and two and so on. On the present view it has been arranging the bricks into patterns. It is most instructive to see what happens when the child is questioned about the result. The adult asks "What have you been building? Is it a tower? Is it Toby's kennel? Is it Daddy's garage? Is it a garden? Is this the wood and this the path to the wood? What lives inside there?" Even though the child is well trained and polite it often makes no reply at all. On the present view, it does not necessarily read meaning into the structures it builds; they are just pure patterns and the child enjoys making patterns and fitting things into them.

Further examples from childhood can be seen in children walking on pavements so as to tread on no joins between

the paving slabs, or alternatively to tread on every join. They can often be seen drawing a stick along uniform railings so as to hit each in passing. Sometimes when walking or running alongside a row of pillars they touch each one without counting, or if they have chalk they may make a 'meaningless' mark on each one. To touch each bannister on the stairs is another frequently observable example. In the slums of towns children can be seen playing a game in which they chalk a pattern of lines on the road or pavement and then apparently have to hop from one part of the pattern to another without putting the other foot to the ground and without treading on the lines. Small children can also be seen repeatedly touching in turn each button on an overcoat or other garment. Swinging and playing at see-saw are examples of fitting into rhythmic patterns, patterns in time, and children do not like the regularity of the pattern to be frequently altered.

Older children fit the parts of jig-saw and other puzzles together and later many of them enjoy putting together the parts of toys specially designed to be put together in many ways. In these, and in fitting together the parts of a working model or machine or watch we are getting to examples in which the whole into which the parts are fitted may have rational meaning, but the action of fitting the parts into the wholes seems to be enjoyed as a pleasant emotion quite apart from the rational utility of the whole, and also quite apart from the superiority aspect of "What a clever boy am I in putting this watch together again so that it will work."

SOME ADULT IRRATIONAL ACTIONS

Before going on to more complex examples it may be noted that some of the wholly irrational* fitting of things into meaningless patterns can be observed in adults. Many men obliged to attend dull meetings, or when thinking alone or

* The word irrational is here used to mean difficult to rationalize or to give 'reasons' for. Patternizing seems to be closely related to the use of pure reason.

talking to a colleague about some problem, can be seen scribbling meaningless patterns on paper and dotting the spaces in the patterns. A man pacing the room in thought will sometimes be seen to be treading on certain uniform parts of a carpet pattern. In the street I sometimes find myself walking along the edge of the pavement so as to keep on the curb-stones, or walking on the pavement slabs without treading on the joins between them, and I am not at all sure that I have not seen scientific celebrities doing it when meditating at British Association meetings, which are incidentally excellent sources of coincidence observations on the behaviour of scientists.

Boswell records an observation on Dr. Johnson in the following passage taken from the *Life*. "He had another particularity of which none of his friends even ventured to ask an explanation. It appeared to me some superstitious habit, which he had contracted early, and from which he had never called upon his reason to disentangle him. This was his anxious care to go out or in at a door or passage by a certain number of steps from a certain point, or at least so as that either his right or his left foot (I am not certain which), should constantly make the first actual movement when he came close to the door or passage. Thus I conjecture: for I have, upon innumerable occasions, observed him suddenly stop, and then seem to count his steps with a deep earnestness; and when he had gone wrong in this sort of magical movement, I have seen him go back again, put himself in a proper posture to begin the ceremony, and, having gone through it, break from his abstraction, walk briskly on, and join his companion." Boswell's care in representing his own view as conjecture is thoroughly scientific. The superstitions hypothesis which is often used to rationalize these irrational acts is here rejected in favour of the pattern conjecture.

Irrational counting provides another type of fitting things into patterns. So meaningless and wholly irrational is this activity that I first hesitated to confess my own practice of this. With me I find it takes the form of counting the divisions of cornice and picture frame mouldings, although

I am not in the least conscious of wanting to know how many divisions there are in the moulding. Moreover, I cannot remember how many there are and I continue to count them when not thinking of the process. At other times I may find myself repeatedly counting objects of any kind even though I have no conscious desire to know their number. At first the only recorded example I could find of this was Napoleon's habit of counting windows when riding into a town. Much as I mistrust the process I was compelled to seek direct information by asking other people. I was surprised to find that everyone I asked gave a positive result of some kind. This irrational counting seems to be widely practised as the counting of steps in a long flight of stairs. Invalids who have to lie in bed for long, count repetitions in patterns of wall papers or fabrics, and hospital patients count the glazed bricks of the bare walls. The only evidence I have that it is also done in childhood is my memory of being told not to repeat to myself what anyone said when they were talking to me. Evidently my lips could be seen moving when I was really counting objects. The moving lips were assumed to indicate that I was repeating to myself what the person was saying. My statements that I was not repeating what was said were not believed, because I could not explain what I was doing. When attention is relaxed the same words of a passage are often read several times almost mechanically.

Another activity similar to irrational counting seems to be irrational touching of objects. An example has already been given in children touching the buttons on their clothes. The similar fidgeting of adults is here regarded as fitting things into patterns, in this case spatial patterns. Superstition is sometimes suggested as an explanation as, for example, in the sixty-fourth chapter of Borrow's *Lavengro*, which gives an account of an author who worries over his books, and touches objects "to avoid the evil chance, impelled by the strange feeling within me" or "to baffle any mischance." Whilst considering the irrational touching of objects, mention may be made of the stroking of animals, stroking a horse, dog, or cat. One element which seems

essential to the pleasure of this is that the part of the hand with which one is stroking shall fit the part of the animal stroked. Unconsciously the hand is so curved that it seems to fit. It is not of course suggested that this fitting is the sole source of the pleasure in stroking the animal; there is pleasure in the warmth, in the smooth texture, and in the attention which the animal gives to us whilst we are stroking it, and so on. Such actions are commonly regarded as trivial or unimportant, but judgments of value are outside the scope of research technique, which is here being applied to observable human action. We are concerned with the fit of actions into the patterning hypothesis, not with the value of any action.

Not only into spatial patterns are human actions fitted but also into patterns in time. The wholly irrational nodding of the head or beating or tapping in time with music is often observed. An interesting example of irrational filling in of what is felt to be a gap is to be found in Gosse's *Father and Son.* The son tells how his father had a "great fondness for singing hymns, in the manner then popular with the Evangelicals . . . so slowly that I used to count how many words I could read silently between one syllable of the singing and another." During my own childhood I used to do a similar thing when hymns were being sung in church. In country churches at least, it seems to be the custom for the organist to wait at the end of each line of a hymn for the dawdlers to finish, before the next line is started. The rhythmic pattern is thus quite spoilt, and I used to try to read to the end of one or more complete lines in the time that elapsed between starting the last note in one line and the first note of the following line. These irrational actions which fit so well into the pattern concept are probably much commoner than might be supposed. Confession of them is probably prevented by the Should-Ought Mechanism and by superiority considerations.

There is a strongly marked tendency to walk in time with a band passing through a street in which we may be walking. Facilitation of long military marches by setting a rhythmic pattern for the footsteps with music or drum

taps is widely used in all countries sufficiently civilized to maintain regular armies. In the article on Military Sounds and Signals in Grove's *Dictionary of Music and Musicians* (Vol. III), an interesting comment occurs to the effect that German soldiers had the habit even as far back as the sixteenth century of fitting doggerel verses to the musical signals. For example to a horn signal:

> "'Kartoffelsupp, Kartoffelsupp,
> Und dann und wann ein Schöpfenkop,'
> Mehl, mehl, mehl."

In this action there is the fitting of a rhythmic pattern of words into the rhythmic pattern of the music, with little regard for the objective meaning of the words. An English example fitted to the bugle Officers' Mess Call is:

> "Oh, officers' wives have pudding and pies
> But soldiers' wives have skilly."

which fits exactly into the rhythm of the bugle call.

All types of dancing are widely practised examples of fitting muscular actions into time patterns. In figure dances or sword dances a spatial pattern is also clearly involved. Military drill, ceremonial formations, and tattoos, beauty chorus dancing of the musical comedy, revue, and pantomime are all examples of action fitted into patterns in time and space. The patterns here chosen are all of child-like simplicity and are pure pattern. A complex example is the Massine ballet *Choreartium* and possibly also in the classical *Les Sylphides.* In ballet of the type of Stravinsky's *Petroushka* both the time and space pattern is highly complex, and moreover cannot be regarded as pure pattern since the whole ballet is an interpretation of a definite series of incidents in the life of Petroushka. It is of interest to note that this analysis of ballet reveals as an element of the spectator's enjoyment, the fit of the movements of the dancers into the rhythmic patterns of the music, and into the spatial patterns of the stage groupings. These are additional to enjoyment of the development of the plot and the

fitness or appropriateness of the music and action to the general meaning. This is an element present also in pure drama, which is free from the rhythmic pattern of music, although fine drama has its own sound patterns of speech. Following the Viennese school of psychoanalysis it is fashionable to regard the pleasure of watching ballet as largely sexual. Such an analysis leaves out of account the pleasure here claimed for the fitting of things into patterns. It would appear that the patterning concept is equally applicable to action in the erotic subject matter of psychoanalysis.

Examples have already been quoted from music of the more static aspect of pattern as shown in such a subject as musical form, and also of the use of music to set a rhythmic pattern into which soldiers or dancers may fit their footsteps. But the actual performance of music provides many examples of actions in which the pattern concept is clearly applicable. Those who play any musical instrument and who are able to read at sight with fluency will know that they can enjoy reading almost any score once (if the score is not too complex for them). Here the player sets himself a pace and then the composer's score represents a pattern into which the player has to fit a very complex series of actions, and there seems to be experienced a distinct pleasure in this process of fitting the actions into the pattern of the music. Readers who read music at sight will know how strong, in reading a score, is the urge not to alter the pattern, once the pace has been set, by slowing down all the actions (Note the superiority element in not wanting to be beaten by the pace). It is a common practice of skilled score readers to omit some detail rather than to spoil the rhythmic pattern. I have verified this fact by observing also professional score-readers. One piece of evidence in favour of the pattern element in score reading is the fact that some people do upon occasion enjoy reading either a Mozart score or a Moszkowski score. Now worshippers at the altar of valuation apply their Should-Ought Mechanisms and say that one ought not to like reading Moszkowski scores if one likes reading Mozart scores, because Mozart's music is better than Moszkowski's. Their analysis of reading musical scores uses

only one superiority idea (i.e. Moszkowski's music is not good enough for me). The same people who play the scores of either composers do, however, also assess values and would all agree that Mozart's music is better than Moszkowski's, and the fact that they do sometimes enjoy reading Moszkowski's music is here analysed into quantitative variations in enjoyment of pattern fittings. To read the Moszkowski Piano Concerto would tax severely the powers of many a concert pianist whilst many amateur pianists could read a Mozart Piano Concerto fairly well. The superiority element is therefore clearly discernible in both cases.

Even if one does not play any musical instrument this element of fitting into pattern can be experienced by anyone who can read a musical score whilst the music is being performed. A number of people whom I have persuaded to use music score when listening to an orchestral concert in the concert hall or by radio or gramophone reproduction, have told me how much they have enjoyed seeing how the music heard fits into the symbols of the printed page. For myself I am unable to enjoy the performance of new music of unfamiliar pattern unless I have a score to help me to see some relation between the part to which I am listening and the whole work, and I prefer to have the pattern of the whole work in my head. The most striking examples of the pattern-fitting element in music are, however, to be found in all types of ensemble performance. When one plays or sings other than solos the fitting of all the performers into the same rhythmic scheme is an essential element in the enjoyment of the activity. If one has an accurate sense of time division it is almost painful to try to play music with any other person who lacks this power. Even if he plays every note of the correct name there is an unpleasant lack of coincidence of the two time patterns. The example of duets on one or two pianos is the most striking illustration because here the complexity of difference in tone quality of the combining instruments found in chamber music is avoided. Antiphonal writing in scores provides much enjoyment to the players.

PATTERN IN GAMES

When the patterning concept first seemed a suitable element for use in an analysis of research technique I tried to think of ordinary actions for which the same concept seemed useful. The first example which occurred to me was the whole group of irrational actions involved in all kinds of games. In all the psychological literature which I have examined these are explained in terms of some kind of *play* instinct, or are not discussed at all. A number of different theories are summarized by Professor McDougall* in a discussion of "some general or non-specific innate tendencies." Professor McDougall himself seems to favour the view that "the human mind has . . . the *tendency to play*," but he does not call this tendency an instinct, and states that "no one of the many varieties of playful activity can properly be ascribed to an instinct of play." Schiller and later Herbert Spencer regarded play as "always the expression of a surplus of nervous energy." According to the Recapitulatory Theory "the child retraverses in his play the successive culture periods of human history, owing to the successive development or ripening of native tendencies to the forms of activity supposed to have been characteristic of these periods." On the theory of Karl Groos the higher (*sic*) animals have a period of youthful immaturity in order that they may play and in so doing become better fitted to cope with their environment.

It is not clear to me how these views help to explain the elaborate games played by even intellectual adults. But all games have rules, and so far as I am aware these rules are not regarded by the players as a necessary evil, but are an essential element in the enjoyment of games. In the patterning hypothesis the rules of a game together with the actions of the other players are continually providing the individual with an uncompleted pattern which his next action tends to complete. A game of chess provides the squares of the chess board into which the players fit the chessmen in accordance with the pattern set by the rules

* *An Introduction to Social Psychology*, chap. iv, 13th edition. 1918.

of the game. Each move is the completing of a pattern and the opponent supplies a clearly defined superiority element since each player tries to win. Card and all other indoor games including ball games such as billiards and ping-pong could be similarly analysed to reveal both the patterning and the superiority elements. Even in such a card-game as Patience, the Superiority element is present although there is no opponent; the player thinks how skilful he is when he gets the game 'out,' and he does not like other people to help him.

An outdoor game such as tennis would be analysed as revealing the pattern element in fitting a ball into the patterns of the tennis court and the gaps left in the defence of the opponents. It is striking evidence of the relevance of the patterning idea as distinct from the superiority element involved in trying to beat the opponents, that a point won in playing tennis is less enjoyed if the ball is not placed in that part of the court at which it was aimed. This might be regarded as being less pleasant from the superiority viewpoint because of failure to do what was attempted, but a number of other players have confirmed my own feelings when I do this. Each is disappointed that the ball was not placed where there was "such a beautiful gap" in the defence. The player wants to put the ball, or the shuttle in Badminton, in a particular part of the court. Merely winning the point without completing what was seen as a gap in a pattern does not give so much pleasure.

Another example of this occurs when a football match is being watched by crowds of spectators. From a certain point of view at any moment in the game a group of spectators may be so situated that they can see better than any others that the conditions are specially favourable for getting the ball through a gap in the defence, and they call out advice to the players with great vigour. Whilst the conditions last, there seems to be a state of anxious strain. The field must look quite different to the players who cannot always be so well placed for seeing gaps through which they could pass the ball nearer to a goal. The advice to "Shoot, man, shoot," called out at football matches is

here analysed as showing the spectators' perception of gaps in the defence and their desire for the filling or the removal of the gaps. In all games where a large field is used the players can never see the whole field as easily as can spectators on the raised stands surrounding the field or pitch. In games such as tennis where a much smaller court is used the players can themselves see the gaps more readily, and each stroke seems to be made to put the ball in some gap in the opponents' defence. Walking and running give other interesting examples where the concept of filling a gap in a a pattern seems useful in explaining the observed facts. If an attempt is made to break a record, that is to cover a distance in an unusually short time, it seems to help to have a pacer—that is a man who keeps ahead all the time. If the athlete is walking he may have a man slowly cycling so as to keep ahead of him all the time. On the present view the pacer is continually keeping open a pattern which the walker is continually trying to complete. Looked at from the superiority viewpoint, the man on the cycle has an unfair advantage, looked at from the pattern viewpoint, the manner of keeping the pattern uncompleted is irrelevant. From my own experience of ordinary non-athletic walking, I seem to like walking along a long straight road or path less than walking an equal distance along a winding road or path. On the long straight road some distant object such as a church tower seems to be used as marking a distance to be walked; it is then found that after a little time the tower still seems to be no nearer. On a winding road a long distance seems to be mentally divided by the walker into a number of shorter distances. On the present view the mere fact of having regarded a turning or a landmark as an object to be reached by walking seems to create some kind mental tension which is not relieved until the object is reached. On the long straight road the walker experiences one long tension whilst on the winding road he experiences a number of alternations of tension and relaxations of the tension. Most people seem to prefer the latter. Furthermore most people prefer, and do in fact choose, walks which make some kind of closed curve, rather

than walk a certain distance and then back again along the same path. The unclosed curve of the circular walk which we set ourselves is, on the present view, accompanied by a state of tension gradually released as the walk is completed. It is noteworthy that in those forms of athletic sports in which no opponent is simultaneously present there is always some spatial pattern before the competitor. Good examples are the high jump and throwing the 'weight.'

All kinds of team work, including that found in games with many players, and rowing in an 'eight,' could be analysed to reveal the element of fitting actions into a pattern. It is not suggested that patterning is the only idea needed to explain the action. The superiority element in being a 'colours' man is readily apparent, and it is well recognized that in some schools Science Masters have been appointed because of their powers to project spheres with suitable velocities at suitable times rather than for their abilities to teach Science to the boys. The postulation of the special idea of team or sporting spirit or instinct is here regarded as an unnecessary complication. Scientists at British Association meetings who, when visiting works, have seen workers continually repeating the same operations may be inclined to agree with the view that these workers are doing actions fitting into patterns. The work would be monotonous if analysed in terms of pure reason. But if the patterning hypothesis is used the workers are not so much making things with rational meaning as irrationally fitting things and actions into patterns.

INCOMPLETED PATTERNS

Reference has been made to a feeling of tension produced when an observer sees something as a pattern with a gap in it, and to a feeling of relaxation experienced when the gap is closed so that all the parts of the pattern fit into their expected places. An excellent way of helping an observer himself to experience this sensation is to get him to observe a mechanical device found in some advertisement signs. The print and design is printed on to plane cardboard,

the surface of which is then cut into a number of discs. Each of these is then rotated simultaneously, but each at a different speed. The relation between the speeds is such that only once in half-a-minute or so do the discs occupy a position when all the parts of the sign fit into their original position when the sign was printed. When the moving sign is approaching this position the relaxation of the tension can readily be experienced so that one is inclined to give a sigh of relief when all the parts of the sign fit together. As the movement progresses and the parts begin to move from their congruent positions the sensation produced is almost painful, especially when the parts fit sufficiently well to enable one to see the normal position of them all.

Some people also experience a sense of relief on seeing a chaotic heap of coins in a bank gradually counted out and arranged into patterns of regular piles, or on putting away drawing instruments, or balance weights, or microscope fittings into the specially made boxes having spaces into which the various articles fit very beautifully. A similar effect can be produced with the models used to illustrate crystal structures. These models, consisting of spheres held together with rods, can be turned about so that in certain positions one sees all the balls lying in a few fairly widely-spaced planes. In other positions of the model their positions look chaotic. There is quite a feeling of relief when one of the chaotic arrangements changes into one showing the fitting of all the parts into a comparatively simple pattern. This same idea may be used to explain the actions of very tidy people who enjoy putting things in their 'proper' places when the 'properness' is irrational. Particular instances of these actions cannot so readily be rationalized as can, for example, the putting of books or scores back upon shelves in alphabetical order of authors.

The typical actions of the collector can readily be analysed in terms of the pattern concept. It is well known that some men enjoy collecting, for example, every edition of a book which has been published in several editions. There may have been changes in the editions, but a collector would not be satisfied with a modern edition of the book in which

all the alterations were noted in footnotes. If he has, say, the first, third, fourth, and fifth editions, he would be especially anxious to get the second edition. There are many card games in which the players have to collect certain groups which are incomplete when the game begins. Children collecting pictures from packets of cigarettes is another example. Sometimes on getting a new picture they do not stop to look at the picture or the letterpress about it, but immediately turn to the number to see if the card is one they want, not so much for its own sake as for its use to complete a series. The pattern concept readily explains the wide popularity of doing cross-word and other puzzles.

Turning to quite a different group of actions, the great popularity may be noted amongst women of many kinds of needlework which consists of outlining or filling in with coloured silks patterns printed on fabrics. Knitting is also very popular. Knitting could be very easily rationalized because the articles knitted are usually definitely useful for thermal insulation of the body, but it is a fact that many women who can easily afford to buy any knitted articles they may want for themselves, or to give to others, often choose to do the knitting themselves. Art needlework is far less easily rationalized. Although the love of colour variation is often present, art needlework is also done in which only one coloured thread is used throughout the pattern, and in lace-making it is comparatively rare to use thread of more than one colour. Ordinary kinds of sewing were not cited first because, although they show the pattern element very well, they can readily be rationalized.

Mention has several times been made of the state of tension or strain which may be felt when something is seen as an uncompleted pattern. This strain is specially noticeable when the pattern is very nearly completed, as, for example, just before the winning-post in a race. I have noticed children who have been touching each pillar or bannister on a staircase, seem very anxious to touch every one if they have already touched nearly all of them. If they are to be stopped they would rather be stopped near the beginning than near the end. In playing ensemble music

the strain is best noticed when one can very nearly but not quite keep up the pace set by the others, in watching ballet when the dancer just fails to synchronize with the music, or when the *corps de ballet* is just taking up a grouping position for a fixed tableau, in collecting when the group is almost completed, in football when a goal is very nearly scored, in drama when some situation is nearly but not quite resolved, in an almost completed puzzle, or in writing an almost completed chapter of a book. A special effort seems often to be made near the end.

In this chapter any kind of task which we have set ourselves has been regarded as an incompleted pattern. Something into which many actions are fitted is seen as a whole. For happiness or contentment in some circumstances, the completion of a task is essential. The nature of the task, whether it be devising a scientific theory to fit certain facts or growing a chrysanthemum as large as a cauliflower, is less important than completing the task. The power to do certain complex actions involves as a necessary condition the power to see a pattern or whole into which the numerous minor actions have to be fitted. Practice in human action does not make perfect unless it is accompanied by a mental concept, the pattern of the task attempted. Without the 'hang' of a thing or the 'knack' of doing it, we have no adequate mental concept of the pattern into which the actions must fit. In making new types of muscular movement, such as are needed in glass-blowing or in learning the technique of a musical instrument, many attempts may be made without improvement until perhaps someone helps us to realize or until we find for ourselves the 'knack' of doing the thing. Once this 'knack' or pattern of what is attempted has been experienced, then improvement with further practice seems to start suddenly. The reader who is a pianist will know that writers upon modern piano technique emphasize strongly the necessity, in practising the technique of the instrument, of getting a clear idea of what one is trying to do; instead of trusting solely to number of repetitions to give the desired result.

The power to see patterns mentally must be supposed

to vary widely according to the individual and the subject matter, and it seems to set a limit to the actions which the individual can do. It must not, however, be supposed that these patterns can be expressed in words or drawings or symbols of any kind. Whether the perception of a pattern consists of seeing rational meaning in the pattern of the action or whole is a matter of definition of terms. I should call a completed oil painting, seen as a whole, a pattern. Although the creator of it realizes that pattern whilst he is painting the picture, he could not express the pattern in words or in any other way than in the completed canvas. Even when he has finished the work we cannot ourselves express in words what the picture taken as a whole means to us. The attempt usually gives statements of what points of detail are liked, or that the picture as a whole gives an impression of perhaps grandeur or sombreness or vivacity. Alternative expression of the pattern is impossible. This leads back again to the same sort of considerations used in discussing a simple geometrical figure in Chapter VII. Pattern cannot be seen to be inherent in the separate parts from which it is built. It is destroyed, or at least it is not expressible, by analysis.

If you have ever learnt to change gears in driving a car, or if you have tried to teach anybody else to do it, you cannot have failed to notice the necessity for seeing the action as a whole. I have noted also that good instrument makers have the power to visualize a complex piece of apparatus as a whole, although they do not understand for what purpose the research worker will be using it. They are able to concentrate upon the details of the separate parts without losing sight of the way they have to fit into the whole. In practice men having this power are rare. A University Science Department is fortunate if it has as many as one mechanic with this power to see complex patterns. As far as I have been able to observe, the ability cannot be taught. A mechanic may be highly skilled in all engineering processes without being able mentally to visualize complex wholes. Such a mechanic has to be given so many separate instructions that he is apt to be overcome by them.

It is not until one has actually to work with such a mechanic that the discovery is made that good mechanics with whom one has previously worked have evidently visualized the same whole as had the research worker. Many of the properties of parts of the whole complex apparatus have been made to fit into the whole, though no mention was made of them by the research worker. This is especially surprising when it is realized that the mechanic does not know what is the research worker's aim.

EXPLANATIONS OF ACTION

In writing this chapter an attempt has been made to show that the pattern concept has also a dynamic aspect useful for correlating a great many actions which, because they cannot readily be rationalized, are usually but little studied. The most popular ways of dealing with them are either to ignore them, or to apply the Should-Ought Mechanism and say they should not be done, or to apply assessment of value to them and say they are unimportant. It will, however, be evident from the chapter on the technique of experiment that the choice of these odd human actions, results from applying to the study of human action the means used in experiment to establish causal relationships. The theory most widely used in explanation of action includes, as basic, the idea that the individual is conscious of a 'reason' for doing the action. If therefore actions can be observed where the individual is not conscious of a 'reason' or where he fails on trying to invent a reason after doing the action, then the 'reasons' theory would fail to fit all the relevant facts. Reflex actions have long been known to be misfits in the 'reasons' theory. The researches of Betcheref, Pavlov, and Watson have extended the kinds of action which seem to be reflex. The patterning hypothesis fits both reflex actions and also actions in which the individual is conscious of a 'reason.'

Another way of explaining action used the idea of something inside, making a man do the action. This type of explanation uses as basic the internal observations which

may precede or accompany action, the wants, appetites, urges, or cravings of which the individual may be conscious, conscience, ambition, impulse, instinct, and the like. In essence this explanation of the individual's action is in terms of the ways in which he can make others act. Not so very long ago the psychoanalytical theories of action were considered very 'modern.' Yet they use this kind of causal explanation of action. Some kind of agent is regarded as getting behind or inside, and pushing or driving a man forward. The idea of the subconscious was devised to contain these agents, devils, or censors in truly mediaeval style. In these diverse ways of explaining action the elements of consciousness (and of the subconscious) are regarded as driving man into action. They are not regarded solely as things preceding, accompanying, or following action, but as causes of action.

In applying the pattern idea to explain action, the causal type of explanation is abandoned. If action is a pattern property, then no causal explanation will serve. No one part more than another of a wireless receiving set in operation can be said to cause the phenomena of wireless reception. Similarly human action is not to be explained causally. A long list of all the external and internal observations accompanying any action may be made, but no one or more of these must be selected and called the cause or causes of the observable action. (The scientific meaning of cause and the technique to be used to establish a cause, are explained in the chapter on experiment.) If a person perceives gaps in a pattern and later fills those gaps, the perception of gaps must not, on the patterning hypothesis, be regarded as something which makes him fill the gaps. There is an observed regularity in the sequence of events, but no other claim must be made.

Near the beginning of the chapter the reader was warned that the facts to be discussed may seem to have little relationship to scientific activities. Their relationship may now be more apparent. To understand scientific classifications, laws, and theories, not only are the characteristics of the finished products examined, but also some characteristics

of the apparatus—the research worker—from which the products issue. If the man be eliminated how will the next scientific fact, classification, law, or theory be got? A sense of dignity, and of superiority-seeking, may demand that lofty analogies be used; a sense of humour may be satisfied with an analogy drawn from a sausage-machine. The characteristics of sausages depend in part upon the sausage-machine and in part upon what is put into the machine. So, characteristics of classifications, laws, and theories depend in part upon facts and in part upon the scientist. Whatever else scientific products may be, nothing is more certain than that they are biological products. Whatever else man may be, he is a patterning animal. If assessment of value be applied, objection may be raised to the word animal. In such circumstances man may be called a patterning biological unit. Whether the Creator is also a patterning unit, and if so, whether his or her or its devices are the same as man's, who knows?

This patterning hypothesis of scientific research relates to classifications, laws, and theories. But the pattern idea has also been used in explaining the scientist's original selection of facts. The biologist's idea of an animal reacting to its internal and external environment is another way of expressing the same idea. The scientist's choice of facts on a particular occasion is regarded as similar to the performance of a radio receiving set in action. Neither the valves alone, nor the condensers, nor the coils of wire alone, nor the electric power supply cause what is observed to happen. The research worker in action is in an everchanging environment. Such an act as recording an observation in a laboratory note-book is regarded as the result of completion of a whole or pattern, of something like an electrical circuit. So long as the man is alive his internal (biological) environment is changing. It continues to change even when he is asleep. Whether he is alive or dead his external environment is also continually changing. All the steady conditions maintained in any living thing are dynamic equilibria. Hence any action, here regarded as the result of completion of one pattern, is not followed

by inaction. New and partly completed patterns are continually being formed.

It is quite evident that the application of this idea of pattern and patterning is a way of explaining things without using the idea of cause. The things here explained are observable facts—the observable actions of scientists. They are effects explained without the use of the idea of causes. In other words, they are effects without any causes of the agent type.

The kind of explanation which the individual finds satisfactory may depend upon such factors as the individual, the subject to be explained, and the occasion. If the idea of explanation is regarded as necessarily involving the discovery of causes, then the patterning theory is not an explanation. I regard all explanation as a patterning process. A scientific explanation is a patterning of facts. But facts, defined as observed coincidences, are only one kind of human experience. In the present state of knowledge no scientific ways of patterning other kinds of human experiences are available. It is unscientific to claim that the scientific patterning devices are more 'natural,' 'true,' or 'fundamental,' unless these words are used with such limitation of meaning as to make their use unnecessary. The observed facts of an eclipse of the sun may be patterned or explained in terms of a dragon trying to eat the sun and being later frightened away by the din of beaten drums, pots, and pans. The same facts may also be patterned scientifically in terms of laws and theories of planetary motion and of optical phenomena. Either type of explanation can be made logically consistent with the facts. Unlike the scientific way, the Chinese explanation does not fit the facts into a larger pattern containing also facts about tides, falling bodies, comets, and seasons. The scientific patterning is found in practice to give a more usable policy of action, but this tells nothing about the 'correctness' or 'truth' of the explanation.

Does the patterning hypothesis imply that scientific classification, laws, and theories are 'arbitrary' patterns of facts? The answer must depend upon what is meant by 'arbitrary.'

On the patterning hypothesis these scientific explanations are neither more nor less arbitrary than are the happenings in the individual components of a radio receiving set in operation. If an arbitrary explanation is one which does not exclude all other logically consistent explanations, then on the patterning theory scientific explanations are indeed arbitrary.

CHAPTER XII

THE SCIENTIFIC THEORY

Of all the devices used by the scientist to pattern facts, the scientific theory is most in favour. Many facts can be patterned into either classifications or laws, but in general scientists try to pattern all facts including those of classifications and laws into still more complex wholes called scientific theories. Many of the facts included in the periodic classification of the elements fit also into the atomic theory. The facts covered by the gas laws fit also into the kinetic theory of gases. The scientist is not content to establish only facts, classifications, and laws.

Although theory is the scientist's most powerful pioneering tool, it is also the device involved in most scientific controversy. Of all scientific products, theories are themselves the least stable and facts the most stable. When agreement between different individuals is taken as basic, any disagreement between scientists must be examined with special care. As a preliminary to the study of the characteristics of scientific theories a survey, in terms of agreement between different individuals, will therefore be made of research technique.

It has been claimed that if facts are to be regarded or defined as impersonal data obtained by the use of sense organs, then they must be obtained by use of those qualities of human observers which in practice lead to universal agreement. Those qualities seem to be the judgment of one object between two others, and the judgment of one event being between two others. These enable agreement to be reached in the judgment of coincidence or the lack thereof. Such judgments give what have been called coincidence observations. Furthermore these observations are not necessarily quantitative or numerical. General agreement between men is reached by this method of observation when once a selection has been made from the whole first observed.

WHEN SCIENTISTS DISAGREE

In this process of selection there is no agreement. It is not a matter of a simple Yes or No, and there are great differences in the power or inclination of men to select this or that from their environment. The following extreme statement by Hamilton is no doubt based on the false idea that facts occur in isolation in nature waiting for men to pick them up. "In physical science the discovery of new facts is open to every blockhead with patience, normal dexterity and acute senses; it is less effectively promoted by genius than by co-operation, and more frequently the result of accident than of design."* The selections give collections of facts or coincidence observations, but scientists are by no means satisfied with accumulations of facts. Whilst few would go to the extremes of Hamilton's statement, most scientists speak of empirical facts as 'merely' empirical.

One method of grouping facts together is based upon the perception of similarities. This again is a process in which agreement is not reached between all men. In the examples, quoted in the previous chapter, of an ardent patriot seeing no similarity between his own country's and any other country's acquiring or retaining of colonies, and of the man who sees no similarity between some of the actions of monkeys and some of his own actions, we have two instances of the great differences in the powers or inclinations of men to see similarities. Often when men state that they fail to see the connection between two things seen by another man, they are genuinely and honestly not seeing the similarity. The fact that they also do not want to see the similarity is irrelevant to the present discussion. If men do not in practice reach agreement in the perception of similarity, there seems to be no rational ground for surprise that they do not reach agreement when they use any devices based upon the perception of similarities. In practice it is found that universal agreement is not reached on

* *Discussion on Philosophy and Literature,* p. 239. London, 1852.

classifications which are necessarily based on the perception of similarity.*

Another way of grouping facts together is by perceiving order in them. This leads to the formulation of natural or scientific laws. The process appears to be based upon the elementary judgment of one thing being between two others and in practice scientists agree about natural laws. There are, however, many groups of facts in which, so far, order has not been seen. It is in such circumstances that the device of hypothesis or theory may serve to group facts together, and the device may even include facts covered also by natural laws. From the following discussion of the hypothesis and theory it will be seen that again use is made of a device in which there is no agreement between different men. To make hypotheses and theories men have to use imagination, and it would be difficult to think of any respect in which men differ more than in the exercise of their imaginations. There are restrictions upon the use or the products of the imagination in scientific research, just as there are in the use or products of the imagination in works of art, but all these activities are based upon a property of men in respect of which they differ widely.

This summarizing of present results in terms of their production of, or failure to give, agreement between different individuals may serve to clarify thought on two points of interest rather to the layman than to the scientist. Any layman who goes one stage further than regarding a scientist as a man who uses test tubes and microscopes in a laboratory, notices firstly that scientists are often changing their theories, and secondly that they sometimes engage in acrimonious discussions in public; that is, they do not always reach universal agreement. In the past until the time of Mach, the Absolute theory of scientific research had been in favour with the majority of scientists and was the only one offered to the layman. He was told that a theory

* It is possible that a fuller and more systematic study of scientific classifications would show that scientists reached agreement on points of similarity and that the differences lie in their powers to devise classifications and to assess their value.

which fitted facts must be regarded as a guess as to what things were really and truly like or how they really behaved. It was reasonable to ask if a theory were true; if it was true, then the theory was a brief description of what the things were in fact really like or how they really behaved. Some critical laymen have been less satisfied with this view than have the majority of scientists. For example, when they are first assured by the leading scientists of the day that light is really a stream of tiny particles moving at high speed, and when in a later age they are assured by equally eminent scientists that light is really not particles but waves in a medium called the aether, and when they are later assured that no such medium as the aether really 'exists,' they may well wonder if scientists are to be trusted as guides to the land of Inner Reality.

There is another puzzling phenomenon; the same men who use one theory at one time and a different theory about the same subject at a later time do, nevertheless, seem to arrive at results or facts which do not seem to change with the theories. Theories come into fashion and theories go out of fashion, but the facts connected with them stay. Photographs can still be taken with a camera whether or no light is really particles or waves, or both, or neither. This same light can be used to irradiate ergosterol in our skin or elsewhere to produce vitamin D and to avoid rickets whether or no the theories of light, chemical action, and disease, are absolutely true or not. The electric power station and the broadcasting station 'work,' although the electron theories about the materials from which they are built change. The chief contributions of science to civilization have so far been facts. The direct applications of science depend upon facts, not upon theories; they depend upon those things in scientific research about which all scientists agree. They depend upon things which have been or could be, in the present stage of knowledge, expressed as judgments of coincidence, and it is solely in these things that agreement between scientists is reached. The discussions, acrimonious or otherwise, in which scientists end by agreeing to differ, are not about facts or coincidence observa-

tions, but about theories and the like into which facts are fitted.

The attention of the lay reader is particularly directed to this kind of disagreement between scientists. It will be noted that even in scientific research, as soon as we depart from coincidence observations, we begin to get disagreement between individuals. That failure to reach agreement with conferences and other such devices often occurs is well known. There is, however, one method not used in scientific research which seldom fails to make the majority of men on the majority of occasions say that they agree. That method is the use of the rack or the revolver. Torture a man mentally or physically, or threaten him enough, and he will express agreement with almost anything. This method is not universally applicable, however, for a few tiresome martyrs will always be found to spoil the universal declaration of agreement; so this method cannot rival in certainty the method of judgment of coincidence used in telling the time indicated by a clock.

There is only one scientific way so far discovered of dealing with differences about facts. That method is by the use of experiment. No amount of discussion alone will settle a difference in judgments of coincidence. To a man who had criticized one of his experiments in light, Newton, in a letter written on November 13, 1675, pointed out this when he wrote that a difference about what can be observed in an experiment "is to be decided not by discourse, but by new trial of the experiment."*

The two points in which the layman is apt to be misled are therefore cleared by noticing that firstly scientists all agree about facts, and that such disagreements as occur between scientists as such are about arrangements of facts. The treachery of words accounts for many disputes and for Schuster's statement: "Scientific controversies constantly resolve themselves into differences about the meaning of words." The second point, that scientists are often changing their theories, will be dealt with in this and the following chapter.

* *Isaac Newton*, by L. T. More, p. 90. London, 1934.

As it seems that all human beings must continually assess values, and that such assessments are apt to introduce prejudice and to distort judgment, I wish to assure the reader at the outset that although I abandon the most fashionable view of scientific theories, I do not therefore belittle the scientific theory. Because I abandon the view that a theory which is logically sound and which fits the facts is a piece of Absolute Truth or a chip off the block of Inner Reality, I do not for that reason value it any the less. I respect facts (coincidence observations) but love the theories which fit them. The object of this chapter is to separate the typical features of theories from their associations with vague metaphysical ideas, associations forced upon the theories by some philosophers, rather than by research workers.

FORMULATION OF HYPOTHESES OR THEORIES

When research workers are devising theories they consider carefully such factors as the logical implications of their postulates and the fit of these with facts, but they concern themselves not at all about inner, outer, under, over, or any kind of 'reality' other than that of the coincidence observation. Facts are such stubborn things to deal with, that vague metaphysical ideas hamper rather than help the formulator of a theory in his task.

The two devices of hypothesis and theory are so similar that they will be treated together. In practice a theory is an elaborate hypothesis which deals with more types of facts than does the simple hypothesis. In the initial stages what is later called a theory is often called a hypothesis. The logical implications of a hypothesis cannot all be seen when the hypothesis is first formulated. When a number of these implications have been worked out and have been found to fit facts then the hypothesis becomes a theory. The distinction between hypothesis and theory is not clearly defined, and the term hypothesis is used very freely. Sometimes hypothesis has been contrasted with opinion. Consideration of facts precedes the formulation of either, but the idea of

testing by comparison with further observation of facts, which is always present when the hypothesis is formulated, is absent from opinion. In so far as the two are comparable, the hypothesis may be said to take that place in scientific research which, in everyday life, is held by the opinion.

In using hypothesis and theory to group facts together the scientist seems to be making a definite change in tactics. The grouping devices so far considered in this book seem to be based fundamentally upon observation of facts. In using them the research worker looks outwards to the external world, selecting some facts and rejecting others. For example, in classifying he selects facts showing common similarities. To classify, he must mentally keep the objects before him; when he has made the selection of data the task is done. In contrast with this, the task of the scientist using the device of hypothesis or of theory has only begun when he has selected his facts. Instead of looking outwards to the external world he seems to look inwards for the completion of his task. Temporarily he seems to retire from the laboratory or observatory, taking his selected facts to the study. There he uses his imagination.

Too little is known of mental science to enable us to understand what happens when a man is formulating an hypothesis or theory. So far as I am aware the whole process is a complete mystery. The process is known to be vitally essential in scientific research. A man who sticks to the *terra firma* of facts, that is to coincidence observations, just sticks. If he will not take the perilous plunge into the world of imagination he will not even find many new facts. It is known furthermore that even men of scientific genius when trying to formulate a theory to fit certain selected facts, formulate many hypotheses and theories which fail to fit the facts, before they devise a successful theory. Quotations from the writings of Faraday, Helmholtz, Planck, and Einstein are given later in support of this statement. It is therefore reasonable to suppose that at present neither the biologists and psychologists nor the men who formulate theories can give any description or explanation of the process.

It is known that so far all originators of theories have

been well primed with the kinds of facts which their theories are designed to fit. But this does not help us to understand the process of formulating a scientific theory, for the postulates which form the basis of a theory are quite unlike any of the facts fitting into the theory, and furthermore it may take a man several years to work out the logical implications of his postulates. It is not until this has been done that he can see whether or no the theory fits the facts. What use is meanwhile made of these facts is a mystery. As a working hypothesis it may be supposed that the knowledge of the facts, like one unit in an electrical circuit, is part of a man's internal environment without which the circuit would be open and the theory would not be devised. This hypothesis, similar to the one used in considering observation, does little more than remind us that each man's internal environment is quite unique, so that we should not expect, for example, any of the physicists who can at once grasp a new theory when it is presented to them, to devise that same theory. This is in accord with experience. It is the origination of the postulates which is quite incomprehensible. The working out of their logical implications and the subsequent modifications of the postulates can be, and often is, done by many men. The fact that so many scientists have the necessary mental ability and background of knowledge to understand new theories at once, and even to work them out, once the postulates have been given, only increases the difficulty of understanding this most vitally essential part of research technique.

CHARACTERISTICS OF SCIENTIFIC THEORIES

Having examined the relevant property of the apparatus which makes theories, attention may next be concentrated upon scientific theories themselves. Those theories in current use which find most favour with scientists appear to me to have the following characteristics in common. The characteristics are first described briefly and are then discussed in detail. They refer to the current practice in theorizing, not necessarily to ideals of theorizers.

(i) *Facts.*—All theories are designed to deal with facts (coincidence observations) *not* with concrete objects and events.

(ii) *Selection of Facts.*—Each theory is formulated to explain only a restricted group of facts chosen by a double process of selection. Firstly, the facts are selected by the observation of concrete objects and events, using the method of judgment of coincidence. Secondly, from all the coincidence observations which can be made with the current experimental technique used upon the given concrete objects and events, a selection only is made for any one theory.

(iii) *Logical Postulates.*—Although the basic ideas, termed the *postulates* of the theory, are got by the use of the imagination, they are not illogical. In practice this means that they are not mutually contradictory or inconsistent with the other rules of logical thought.

(iv) *Clarity.*—The postulates are clear, not vague. In practice this means that the postulates are so stated that the originator of the theory and also other persons with the necessary technical knowledge can apply pure reason to them. Although for brevity the postulates may contain technical terms, they are not stated so that the reader is "stunned with dark and empty words" (to borrow a phrase from Robert Boyle). In those theories where the application of pure reason to the postulates takes the form of mathematical treatment, the concepts expressed in the postulates are not necessarily such as can be visualized. Hence the word 'clear' here means 'such as can be reasoned about,' not necessarily 'such as can be visualized.'

(v) *Simplicity.*—The postulates are as simple and few as the theorizer can make them.

(vi) *Tendencies.*—The postulates are not expressed in terms of tendencies to do or be the things *which* the theory is devised to explain.

(vii) *Coincidence Observations Absent from Postulates.*—The postulates are not statements about coincidence observations; they are not made about things which can be seen or handled or which can give any other sense data.

(viii) *Absurdity.*—The postulates may appear absurd or

contrary to common sense. Provided they are logically se
consistent, their appeal or failure to appeal to the sen
of humour or to common sense is quite irrelevant
theory making.

(ix) *Diversity.*—When a theory is devised to deal wi
very diverse and varied facts, usually at least one factor
postulated as subject to *quantitative* variation.

The theory is developed by applying pure reason to t
postulates, and the final results can always be express
in terms of things observable by the judgment of coincidenc

(x) *Fit.*—The logical derivations from the postulates
the facts. When the mental processes called pure reas
are applied to the mental concepts or ideas called t
postulates of the theory, to give things expressible as coi
dences, then these agree with coincidences derived fro
observation of concrete objects and events. That is to sa
mentally derived coincidence observations agree with the
got by the aid of sense organs.

(xi) *Prediction.*—Reasoning about the postulates som
times leads to the search for facts (coincidence observatior
other than those first considered when the postulates we
formulated, and these predictions are fulfilled.

It will be noted that theorizing, as practised in scienti
research, begins with facts, proceeds with the mental devic
of imagination and logical reasoning, and ends with fac
The theory is a mental structure and the facts which
into this structure are got by the use of sense organs.
do not wish to imply that no mental processes are us
in getting coincidence observations by the use of ser
organs.) These characteristics of theories will now
examined in more detail.

(i) *Facts.*—All theories deal with facts, *not* with things th
can be seen or handled, that is, they do not deal wi
concrete objects and events. The Kinetic Theory of Gas
one of the current scientific theories which by scienti
is ranked high in value, may be used to illustrate t
characteristic of theories. It is well known that the substan
called nitrous oxide or laughing gas is a gas and has be

used by dentists as an anaesthetic. It is well known that chlorine is a yellowish coloured gas which has been used in warfare to kill or injure men. It is well known that coal gas is colourless and has been used for committing suicide. Nobody doubts these facts. Nitrous oxide, chlorine, and coal gas under these conditions are undoubtedly gases, and yet the Kinetic Theory of Gases tells nothing whatever about these selected facts. It does not help us out of the difficulty if we apply assessment of value to modify the statement of this first characteristic of theories, so that it may read 'Scientific theories deal only with the important facts about concrete objects and events.' Although the word important used in an absolute sense is typically vague, I doubt if any reader of this book would be willing to stretch its meaning so far as to account these particular properties of nitrous oxide, chlorine, or coal gas unimportant if they were applied to him. If, however, specimens of these gases were examined and other facts about them were selected, then a number of facts could be found which would fit into the theory. These three gases are not themselves exceptional in so far as the Kinetic Theory of Gases is concerned. Some of the facts about nitrous oxide, chlorine, and coal gas fit into the theory. Furthermore, some selected types of facts about all gases do not fit into the theory.

What is true of this particular theory is true of all theories. In general they deal with abstracted data, not with the objects and events from which the data were obtained or may still be obtained by observation.

(ii) *Selection of Facts.*—Poincaré wrote: "An isolated fact can be observed by all eyes; by those of the ordinary person as well as of the wise. . . . Facts are sterile until there are minds capable of choosing between them." Since theories deal only with selected facts it may well be asked what factors determine the initial selection. In so far as I am aware, nothing whatever is known of these factors. It is known that men who make theories are saturated with the facts of their particular science, but how they come to decide to spend perhaps years in the formulation of a theory to fit certain types of facts and not other facts is a complete

mystery. It has been said of one famous living scientist that he has 'a nose for a result' or that he 'can smell a result.' He works upon experiments which always give results of timely interest, and he does not do experiments which give no results of special interest in his science. In the present state of knowledge, no more can be said of how successful theorizers come to select facts which they find, perhaps years later, can be fitted into a theory, than that they can 'smell' a theory in a group of facts. This statement is of course a roundabout way of saying that I do not know.

Although a definite group of facts is first selected, it is often found later that other facts can also be fitted into the same theory. A theory is in practice not necessarily restricted only to those facts first considered. Some of these even may have to be omitted.

Although these first two characteristics of theories are closely related I have thought it best to divide them, the better to emphasize that theories do *not* deal with concrete objects and events.

(iii) *Logical Postulates.*—Not only is the original selection of facts a complete mystery, but so also is the formulation of the postulates. The sole restriction in this process is that they shall not be illogical. In practice this seems to mean that they shall not be mutually contradictory. The expenditure of much labour and time are necessary before their suitability can be determined, so that in the first stages the theorizer's imagination may run riot provided the results are logical.

USE OF IMAGINATION

The necessity for the use of imagination is emphasized by many scientific writers. Huggins wrote: "This creative use of the imagination is not only the fountain of all inspiration in poetry and art, but is also the source of discovery in science, and indeed supplies the initial impulse to all development and progress. It is this creative power of the imagination which has inspired and guided all the great discoveries in science." Max Planck, the originator of the

Quantum Theory in physics and one of the most famous theorizers of the day, writes thus of hypothesis:* "It must be free from everything in the nature of a logical incoherence, otherwise the researcher has an entirely free hand. He may give rein to his own spirit of initiative and allow the constructive powers of the imagination to come into play without let or hindrance. This naturally means that he has a significant measure of freedom in making his mental constructions . . . this freedom . . . is a constructive application of the imaginative powers. It is not a mere arbitrary flight into the realms of fancy." The physicist "must select according to a plan which will in the first instance be hypothetical and therefore a construction of the imagination. And when he finds that the given results will not fit into one plan he discards it and tries another. This means that his imaginative powers must always be speculating." Planck states quite clearly that "The line of thought which leads to" various alternative hypotheses "has its origin entirely outside the ambit of logic." In order that a physicist may be able to devise useful hypotheses he must have two characteristics. "He must have a practical knowledge of his whole field of work and he must have a constructive imagination."

The vital necessity of the use of the imagination is clearly brought out in these statements by one of the most brilliant scientists of the day. Although his work proves that he can make hypotheses, his statement implies quite clearly that he cannot explain how he does it. Speaking of the choice of a suitable hypothesis when many are available he says, "As to how one may reach a decision in the midst of this uncertainty, no general rule of procedure can be laid down." He suggests that "it would be an interesting mental exercise if one were to take as many as possible of the hypotheses which have proved significant of results in the pursuit of physical science and then try to discover the respective combinations of ideas to which the hypotheses owed their origin. But the task would be a difficult one because, generally speaking, creative master minds have felt a per-

* The following extracts are from chapter iii of his *Where is Science Going?* London, 1933.

sonal aversion from the idea of unfolding before the public gaze those delicate threads of thought out of which their productive hypotheses were woven, and the myriad other threads which failed to be interwoven into any final pattern."

The following statement by Leibnitz in the opening paragraph of *Historia et Origo Calculi Differentialis* (1684) bears on this question. "It is an extremely useful thing to have knowledge of the true origins of memorable discoveries, especially those that have been found not by accident but by the dint of meditation. It is not so much that thereby history may attribute to each man his own discoveries and others should be encouraged to earn like commendation, as that the art of making discoveries should be extended by considering noteworthy examples of it."

The reference in Planck's statement to 'a personal aversion from the idea of unfolding before the public gaze, etc.,' might lead to the suggestion that men of scientific genius do know how they came to originate hypotheses but do not like to explain the process. As evidence against this suggestion the following statements by men of scientific genius are given to show that they all freely acknowledge that they spend a great deal of time and effort on ideas which they later reject. From this it is reasonable to suppose that even the men who make successful hypotheses do not know how also to avoid making unsuccessful ones.

As both Planck and Helmholtz mention, men are reluctant to give in public detailed accounts of their failures. (Query: What would the psychoanalysts have to say about this fact?) Moreover, publications are nowadays reduced to the bare minimum necessary to give the results established by a piece of research. It is doubtful if the logical presentation of results given in any research paper ever published represents also a chronological account of what the scientist did when he was carrying out the work. Research papers are not written for the use of students of scientific method. In any of the sciences the number of papers published* is so

* In 1934 the number of research papers published may be estimated from the number abstracted in that year. Some examples are Pure Physics (*Science Abstracts*, Sect. A), 5,269, Applied Physics (*ibid.*,

great that a research worker cannot now read them all and still have time for the practice of research. The necessity for brevity, quite apart from any other considerations, precludes the publication of detailed accounts of the way in which any piece of research is actually done.

Sometimes a man of genius, such as Einstein, will give in a special lecture, or in a letter, or diary, some account of the lines along which a research has progressed, but nowadays this is quite exceptional. The following extracts taken from such sources are given in support of the view that even men of scientific genius do not know how to restrict their activities in hypothesis-making to those which lead to successful theories. Faraday wrote: "The world little knows how many of the thoughts and theories which have passed through the mind of a scientific investigator have been crushed in silence and secrecy by his own severe criticism and adverse examination; that in the most successful instances not a tenth of the suggestions, the hopes, the wishes, the preliminary conclusions have been realized."

Helmholtz* writes: "In 1891 I have been able to solve a few problems in mathematics and physics including some that the great mathematicians had puzzled over in vain from Euler onwards." He mentions some of the problems and continues: "But any pride I might have felt in my conclusions was perceptibly lessened by the fact that I knew that the solution of these problems had almost always come to me as the gradual generalization of favourable examples, by a series of fortunate conjectures, after many errors. I am fain to compare myself with a wanderer on the mountains, who, not knowing the path, climbs slowly and painfully upwards, and often has to retrace his steps because he can go no farther—then, whether by taking thought or from luck, discovers a new track that leads him on a little,

Sect. B), 2,992, Pure Chemistry (*Chemical Abstracts*, Sect. A), 15,081, Applied Chemistry (*ibid.*, Sect. B), 14,325, Biology, pure and applied but excluding clinical medicine (*Biological Abstracts*), 3,052, Psychology (*Psychological Index*), 5,824 in 1932. About 36,000 scientific journals are current.

* See *Life*, by Königsberger, p. 180.

till at length when he reaches the summit he finds to his shame that there is a royal road, by which he might have ascended, had he only had the wits to find the right approach to it. In my works I naturally said nothing about my mistakes to the reader, but only described the made track by which he may now reach the same heights without difficulty."

Planck's statements including the reference to "the myriad other threads which failed to be interwoven into any final pattern" have already been quoted. Those of Einstein occur in a lecture given at the University of Glasgow in 1933.* They are difficult to quote in brief extract, but it may be noted that the Special Theory of Relativity was published in 1905, and the lecture deals with the development of the General Theory which appeared ten years later in 1915. Details are given of both unsuccessful and successful hypotheses used in that period. He stated, for example, how at the beginning "Like most physicists, at this period I endeavoured to find a 'field law.' . . . Investigations on these lines, however, led to a result that caused me grave misgivings." After giving reasons, he adds: "I gave up, therefore, the attempt, which I have sketched above." He explains other lines of attack followed from 1908 until 1911, and was apparently not satisfied until about 1912. Towards the end of the lecture he refers to "errors in thinking which caused me two years of hard work before at last, in 1915, I recognized them as such." The lecture ends: "The final results appear almost simple; any intelligent undergraduate can understand them without much trouble. But the years of searching in the dark for a truth that one feels, but cannot express, the intense desire and the alternations of confidence and misgiving, until one breaks through to clarity and understanding, are only known to him who has himself experienced them." From the evidence of these four men of genius it seems reasonable to suppose that the process of formation of scientific hypotheses is at present a complete mystery. Even with such men in whom the hypothesis

* A. Einstein, *The Origin of the General Theory of Relativity*, Glasgow: Jackson, Wylie & Co., 11 pp. 1933.

birth-rate is excessively high, it only just manages to exceed the hypothesis death-rate by a narrow margin.

It would be out of accord with practice to suppose that every scientific hypothesis which is rejected has been a complete waste of time and effort. Even if the research worker's pleasure in working out the implications of a hypothesis is ignored, it must be remembered that many a good experiment has been done on the basis of an hypothesis which was later rejected. As de Morgan put it: "wrong hypotheses, rightly worked from, have produced more useful results than unguided observation."*

The non-scientific reader who knows that there are at the present time about 36,000 scientific journals each publishing many researches every year, may well wonder how enough hypotheses are found to go round. He may well doubt either the necessity of hypotheses in research work or the difficulty of making them. The explanation is that one hypothesis may provide research work in many fields. For example, in 1912 Laue formed an hypothesis which would imply that X-rays and crystals would interact in a similar way to diffraction gratings and light. Friedrich and Knipping acting on this idea carried out an experiment with X-rays and a crystal which gave new results fitting in with Laue's hypothesis. The Braggs formed an hypothesis that this result could be used to determine the fine structure of crystals. That is, by the creative use of the imagination they showed how to correlate the vast number of facts about the properties of crystals and the still greater number of facts included in the Atomic Theory of Matter. The effect of this pioneering work can be seen in Fig. 27 where the growth of research papers on X-rays and crystal structure is plotted. The data are taken from a bibliography due to Wyckoff.† The graph must not be regarded as representing quantitative results. It is given as a concise summary of data. The first part of the curve is not representative because of the retarding effect of a war upon research work which at the time cannot be seen as useful in military offence or defence. The war

* *A Budget of Paradoxes*, vol. i, p. 87.

† *The Structure of Crystals*, 2nd edition, pp. 397–475. New York, 1931.

lasted from 1914 to about 1918–19. The latter part of the curve showing a decrease in the number of researches is not representative, as the bibliography is there incomplete. Each year the amount of research done in the subject is increasing. The greater part of the creative imagination was supplied by the founders of the subject, the later develops

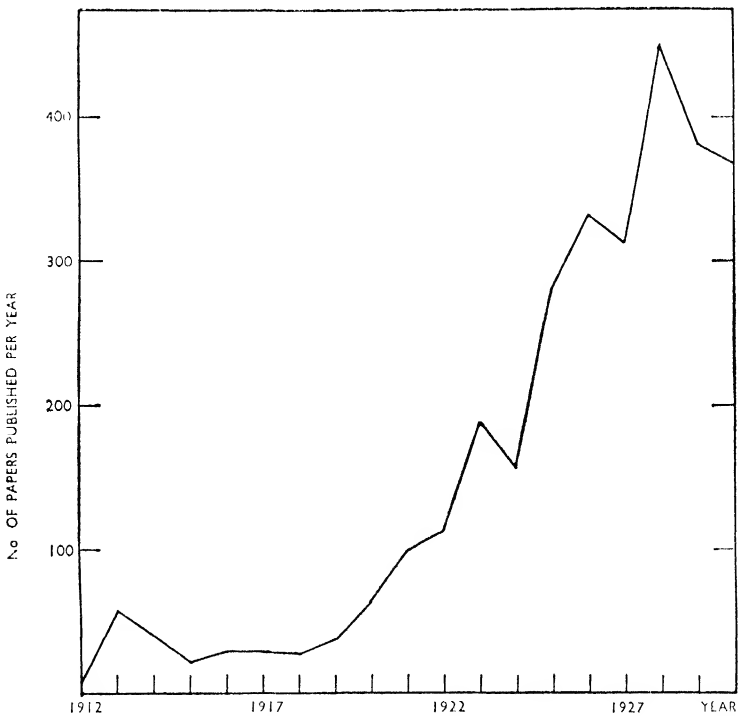

FIG. 27.—Diagram showing the growth of one branch of research in physics (X-ray crystallography) from its origin in 1912

ment being concerned chiefly with the logical working out of the implications of the original hypotheses and their comparison with experimental results.

Although the postulates of a theory are never mutually inconsistent or self-contradictory, the postulates of two theories covering related fields are sometimes mutually inconsistent, as, for example, in theories of light. Newton's

Corpuscular Theory fits some of the facts and Huygen's Wave Theory fits others. The two theories are both logical but are based upon postulates which, as stated, are mutually contradictory. In 1911 Sir William Bragg said: "For the present we have to work on both theories. On Mondays, Wednesdays, and Fridays we use the wave theory; on Tuesdays, Thursdays, and Saturdays we think in streams of flying energy quanta or corpuscles." This passage is frequently quoted; when it is used by non-scientific writers their comments seem to suggest that bewilderment which so often results from regarding theories as identical with the facts fitting into them. The continuation of the passage, expressing with its writer's characteristic clarity the research worker's attitude, reads: "We cannot state the whole truth since we have only partial statements, each covering a portion of the field. When we want to work in any one portion of the field or other, we must take out the right map."* He expressed a similar sentiment in the following passage of his British Association Presidential Address on *Craftsmanship and Science* (1928), "Here is an actual case where the human mind is brought face to face with its own defects. What can we do? What do we do? As physicists we use either hypothesis according to the range of experiences that we wish to consider. . . . We know that we cannot be seeing clearly and fully in either case, but we are perfectly content to work and wait for . . . understanding."

(iv) *Clarity.*—The postulates of an hypothesis or theory are stated in clear, not in confused or vague terms. Now what is clear to one person may not be clear to another. In the first instance postulates are stated in words, and as Bacon put it, "words, like a Tartar's bow do shoot back upon the understanding and mightily entangle and pervert the judgment." An example of this occurred in the discussion of observation. It was there seen that attempts at verbal interpretation of, for example, Quantum Physics can become almost hopelessly confused by the treachery of words. Some writers of popular scientific books have supposed that the word 'observable' has, in quantum

* Robert Boyle Lecture, "Electrons and Ether Waves," p. 11.

mechanics the same meaning as in everyday usage. On the other hand, if unfamiliar words such as, for example, *entropy* or *libido* were used in the statement of postulates, then these statements would not be clear without further explanations. In practice the postulates of theories often contain new ideas for which the current language has no adequate words and new words have to be coined or old words used with new meanings. For clarity the coining of new words is often an advantage. On seeing for the first time a word such as *entropy*, we are forced to give careful attention to its meaning. We are, however, apt to pay no special attention to any restrictions a writer may be putting upon the meaning of so common a word as *observable*. When Bacon was writing of the obstacles in the application of pure reason to problems he called these obstacles Idols and regarded "Idols imposed by words on the understanding" as "the most troublesome of all." He pointed out the great advantage gained when we "imitate the wisdom of the Mathematicians."

In research the language of mathematics is used wherever possible, and greatly helps in the application of logical reasoning. From the present point of view, the great advantage of the use of mathematical symbols is that it tends to force the user to express his meaning clearly. Although certain conventions are commonly adopted by mathematicians, in general it may be said that not even the most brilliant mathematician alive, when starting to read a new piece of mathematics, can say what is the meaning in that work of such a symbol as x. Such a symbol has no particular meaning until its user has given it one; as Whitehead puts it, "a symbol which has not been properly defined is not a symbol at all. It is merely a blot of ink on paper which has an easily recognizable shape."* It follows that a user of mathematical symbols is forced at the onset to write down a section usually headed *Notation*, in which he explains what each of the symbols will be taken to represent. The advantage from the point of view of clarity is evident. To compare this practice with the use of words, it is as if each writer were forced to give a glossary at the beginning of each work.

* *Introduction to Mathematics*, p. 91.

There is a further advantage in the expression of postulates in mathematical symbols. From one point of view the technique of pure mathematics may be regarded as a highly systematized technique for logical reasoning or for applying pure reason. Hence it follows that once postulates have been written in mathematical language, many men with the necessary mathematical technique can be found to work out their logical implications, and the technique is such that they will be able to reach agreement even though they work quite independently.

It is unfortunate for clarity that the majority of things in which the majority of men are interested on the majority of occasions have not, so far, been expressed in mathematical language. When mathematical symbols cannot be used in the statement of postulates, these have to be so expressed that at least scientists with the necessary technical training can apply pure reason to them, to work out their logical implications. It is in this sense that the word 'clear' must be understood when we assert that the postulates of an hypothesis or theory are clear. Most scientists dislike postulates which are so vague or elastic that almost anything within a certain range of facts can be logically deduced from them. For example, some postulates used in certain theories of human behaviour have been criticized on the grounds that because of their indefiniteness they can be used to explain almost any kind of behaviour. In each new case the meaning of the postulates has almost entirely to be reinterpreted.

From such statements as that of Heisenberg, quoted in Chap. XIII (p. 258), it is evident that the particular implications of the postulates of a theory required to explain some particular fact would not necessarily be deduced by the theorizer if he did not know the fact beforehand. This does not, however, imply that such postulates are vague. Postulates would be called vague or not clear, and would be disliked by scientists, if they had to be given new meanings for each new fact explained by their aid. In vague postulates the reader is "stunned with dark and empty words" such as were used in the older explanations of natural phenomena.

The word 'perfect' is a typical example. The Aristotelian explanation of planetary orbits was 'Planets move in circles because the circle is the only *perfect figure*.' The present-day dissatisfaction with this type of explanation is on the grounds that if it be postulated that planets appear to move in perfect figures, then one cannot deduce the shape of the planets' orbits by applying pure reason to this postulate. In a more modern explanation, such mental concepts as mass and gravitation are used, and it is postulated that two planets may be regarded as attracting each other with a force which is directly proportional to the product of their masses and inversely proportional to the distance apart of their centres of mass. (Note the brevity of a mathematical statement of this postulate: $G = \gamma\, m\, m'/d^2$.) From this the shape of the orbit can be deduced by logical reasoning, whereas with the older postulate the prediction of the shape of the orbit has to be 'faked' or 'cooked' by saying that the circle is the only perfect figure. Furthermore when this latter statement is made there is no necessity for a chain of logical reasoning between the postulate and the result.

Another typical example of a vague hypothesis is Kepler's colour hypothesis given in his Proposition XV as "Colour is potential light buried in pellucid matter." An example where both postulate and result are vague is the statement by Roger Bacon (1214–1294) that "Gold is the most perfect of metals because in it Nature has finished her work."

VISUALIZATION

In the majority of scientific theories the postulates are such that pure reason can be applied to them without the use of mathematics. The postulates deal with mental concepts, but usually it has been possible to visualize them. For example in the atomic theory of matter, the postulated atoms could be visualized as tiny spheres. More recently, theories have been used in physics in which it is not possible to visualize the processes. In older theories such as, for example, the kinetic theory of gases, the postulates were such that the logical implications could be worked out quali-

tatively by visualization (that is, by picturing in the mind's eye what was going on), and quantitatively by the use of mathematics. Because this double process is not possible with the newer theories in physics it does not follow that recent theories in physics use vague postulates. It is well known that human beings vary greatly in the acuity of their different senses. Presumably visualization depends upon vision, and there would appear to be no logical necessity to insist upon the use of vision as an aid in reasoning.

Many devisers of new experiments have used visualization of the processes described by the current theories of their time. Some workers are apt to regard the newer theories as vague or as "weirdly ingenious modes of expression."*

Probably all experimental workers use visualization, and it is evident that a theory whose concepts cannot be visualized cannot help workers to devise new experiments by the process of visualization. The process of visualization in devising or interpreting new experiments is, however, very mysterious, as is evident from the following statements of Faraday. He had been working at experiments on the influence of a magnetic field upon polarized light and had published the results in the *Philosophical Magazine* in 1845. In the following year he published a letter in the same journal setting forth his light hypothesis in considerable detail. He states: "The view which I am so bold as to put forth . . . endeavours to dismiss the aether, but not the vibration. The kind of vibration which, I believe, can alone account for the wonderful, varied, and beautiful phenomena of polarization . . . are lateral. It seems to me, that the resultant of two or more lines of force is an apt condition for that action which may be considered as equivalent to a *lateral* vibration, whereas a uniform medium like the aether does not appear apt, or more apt than air or water." He points out that waves in air or water "are direct, or to and from the centre of action," and that a uniform medium such as the postulated aether would propagate similar waves. He therefore dismisses the aether and seems

* Lodge, *Nature*, cvi, p. 796. An article dealing with the Relativity Theory.

to be left with a system of vectors, a kind of geometri picture. With this he is able to co-ordinate the coincider observations obtained by experiments with polarized lig As I understand the statements, Faraday is satisfied w some kind of visual picture which fits the facts, but does not make a visual model like that of some concr object in the laboratory. He does not seem to mind c missing "the aether but not the vibration." From repeated statements elsewhere that "we may be *sure* facts, but our interpretation of facts we should doubt," a from my own prejudices against all claims to absolute tru I think that he is using the process of visualization as a t in research without worrying about making a visual mo to represent the 'real truth' about light. In a footnote whi he added later to the 1845 paper he speaks of "Neitl accepting nor rejecting the hypothesis of an aether, or t corpuscular, or any other view that may be entertained the nature of light; and as far as I can see, *nothing be really known* of a ray of light more than of a line of magne or electric force, or even of a line of gravitating force exc as it and they are manifest in and by substances." (T italics are not in the original.) These statements of Farada are given in support of the view that even when the pos lates of a theory can be visualized, the processes of visu zation are hardly less 'weirdly ingenious' than are th used in mathematical theories in which visualization impossible.

(v) *Simplicity.*—The postulates of a scientific theory a as simple and few as the theorizer can make them. This only a special case of what might be called the Simplic Rule in scientific research. Wherever a research worl has a choice between the simple and the complex he ter to choose the simple, whether it be in a method of carryi out an experiment or in reasoning about the results.

The fate of various theories of the solar system serves a good example of this tendency to choose the simpl theory which can be made to fit the facts. For fifte hundred years the generally accepted theory of the planeta motions was Ptolemy's. He appears to have arrived

this Perfection Theory by the aid of the Should-Ought Mechanism. Planets moved at uniform speeds in circles because the circle was the most perfect figure. Tycho Brahe used judgment of coincidence and found a lack of fit of the facts with Ptolemy's theory. (Copernicus regarded the sun as the centre of the system but still thought that "the movement of heavenly bodies is uniform, circular, perpetual, or else composed of circular movements.") Tycho Brahe "developed a system of his own, which was in some sort a compromise between the Ptolemaic and the Copernican systems. . . . This cosmical scheme . . . may be made to explain the observed motions of the heavenly bodies, but it involves a much more complex mechanism than is postulated by the Copernican theory."* At this stage there was a choice between two theories which each fitted the facts. The simpler theory survived and was later replaced by the still simpler theory of Newton.

This search for simplicity seems to be connected with the subjects discussed in the chapter on pattern in action. There it was suggested that the seeing of disorder or complexity produces a state of tension in the observer, which is not released until the disorder or complexity is changed to order or simplicity, or until the attention is taken from the subject matter concerned. From the results of such internal observation it is suggested that the kind of behaviour in which a scientist may be described as seeking simplicity, can be related to general behaviour. This same idea of tension produced by the perception of disorder or complexity can be used to explain the unpleasant and baffling experience of trying to think of too many types of facts or ideas at once; this latter experience was discussed in the treatment of classification and the whole of these phenomena are regarded as illustrations of the use of the pattern concept in action. From this point of view actions which can be interpreted or analysed in terms of the concept of simplicity-seeking, can also be analysed in terms of the pattern-making or completing concept. Actions which can be interpreted as the pursuit of simplicity can also be

* H. S. Williams, *History of Science*, vol. ii, p. 64.

interpreted as the pursuit of pattern, and in this latter case they are seen as related to general human action.

Quite a different interpretation of simplicity-seeking actions is given in the Absolute theory when the simpler of two theories which are logically sound, and which each fit a certain group of facts, is regarded as nearer to absolute truth or as "more likely to be true." In discussing his first rule of philosophizing Newton puts it: "Nature is pleased with simplicity." Keill says: "Nature always proceeds in the simplest and most expeditious method; because by this manner of operating, the Divine Wisdom displays itself the more."*

In rejecting this application of the Absolute theory I analyse the facts as follows. It is well known that men tend to make their Gods after their own images, as, for example, when a mathematician says: "The Great Architect of the Universe now begins to appear as a pure mathematician." Furthermore if man is to avoid confusion of thought he must have simplicity. Since men try to avoid conscious confusion these two facts may be combined to lead to the expectation that a man's conception of a God will be that of a Being who, like himself, must have simplicity. Simple laws of nature and simple theories are therefore more god-like, that is, they are nearer to Absolute Truth than are complex laws and theories. It will be seen that my speculative explanation of the simplicity rule in scientific research is in terms of the concept of pattern, and that my explanation of the claim that simplicity is next to godliness, is in terms of the concept of Superiority-seeking.

When the simplicity rule is applied to choice of hypotheses it is related to the following classical examples of rules of systematic inquiry. William of Occam's rule, known also as Occam's Razor, was *Entia non sunt multiplicanda praeter necessitatem.* Hamilton expressed it: "Neither more, nor more onerous, causes are to be assumed than are necessary to account for the phenomena." Newton's version, given in his Rules of Philosophizing at the beginning of the third book of the *Principia*, reads: "Rule 1. No more causes of

* Keill, *Introduction to Natural Philosophy*, p. 89.

natural things are to be admitted than such as are both true, and sufficient to explain the phenomena of those things." Related to these rules is Descartes' resolution "to conduct my thoughts in such order that by commencing with objects the simplest and easiest to know, I might ascend by little and little, and as it were, step by step, to the knowledge of the more complex."*

Although the practice in research is to choose the simplest theory or hypothesis which will fit the facts, it is also the practice to frame theories to fit as many types of fact as possible. These two aims are often mutually incompatible. A famous example is the contrast between Newton's and Einstein's theories covering gravitational phenomena. In a passage already quoted Einstein says: "The final results appear almost simple; any intelligent undergraduate can understand them without much trouble." But Einstein's theory is far more complex than Newton's and his 'any intelligent undergraduate' belongs to the same class as Macaulay's 'every schoolboy.' However, Einstein's theory fits more types of facts than does Newton's and includes all the facts covered by Newton's. It gains in comprehensiveness but loses in simplicity. In choice of hypotheses, then, the simplicity rule applies only to hypotheses covering the same range of facts. In general the rule applies to postulates rather than to complete theories.

(vi) *Tendencies.*—The postulates of a theory are not expressed in terms of tendencies to do or be those things which the theory is designed to explain. I think that the general use in postulates of the word 'tendencies' will become less and less common in the future because its use depends upon the false idea that theories deal with concrete objects and events. As already explained in section (i) of this chapter, theories never deal with concrete objects and events, but only with selected facts. Where this dis-

* *Discourse on Method*, Leyden, 1637; Everyman's Library Edition, pp. 15–16. A note on Occam's Razor is given in Karl Pearson's *Grammar of Science*, pp. 392–3 of the third edition, London, 1911. He refers also to a valuable historical note in Sir William Hamilton's *Discussions on Philosophy*, 2nd edition, pp. 628–31. London, 1853.

tinction has not been realized theorizers have endeavoured to get over the difficulty by expressing their postulates in terms of tendencies. In formulating psychological theories of human action, such a postulate as 'people tend to put things in their mouths' could not be used to explain feeding, but a tendency to act so as to preserve life might formally be postulated to explain the action of feeding. In the various Instinct Theories of human action 849 types of instinct have been postulated and some of these are of the type, inadmissible in a scientific theory, similar to the example just quoted.*

(vii) *Coincidence Observations Absent from Postulates.*—The postulates of a scientific theory or hypothesis are not nowadays made in terms of things which can be observed by judgment of coincidence. For example the atomic theory of matter used the concept of atoms, the electron theory of atoms used the concept of electrons, an electro-magnetic theory of light used the concept of an aether, theories of human or animal action use such concepts as instincts, tendencies, urges, purpose, motive. All of these are mental concepts which cannot be observed by the coincidence method. Similar mental concepts, which cannot be observed by judgment of coincidence, but which are used in theories, are force, energy, entropy, gravitation, surface tension, viscosity, elasticity, particle, rigid body, and the many other concepts which have proved so serviceable in physics.

In classical physics a theoretical treatment meant a mathematical treatment, and many of the mental concepts used in these theories were simple abstracts such as the particle of dynamics, and it was often possible to picture the postulates. As already explained in Chapter IX, although such abstract ideas can readily be visualized they cannot be observed by judgment of coincidence. They can be seen by the mind's eye but they cannot give coincidences. This seventh characteristic of theories therefore applies equally to classical or modern theories.

* For details see L. L. Bernard, *Instinct*. London, 1924.

CHAPTER XIII

THE SCIENTIFIC THEORY (*Continued*)

In this chapter the remaining four characteristics, already enumerated, of scientific theories will be discussed.

(viii) *Absurdity.*—Provided the postulates of a theory are logically sound it does not matter if they appear absurd or contrary to common sense. The two most recent theories in physics give striking examples of this, for the ideas of the Quantum and Relativity theories were so unlike the ideas used in classical physics that they appeared contrary to common sense. The quantum theory originated in a hypothesis or theory devised by Max Planck to explain certain phenomenon of radiation of heat which had not been explained in terms of classical mechanics. Referring to this first communication to the German Physical Society on December 14, 1900, Jeans* has written: "Not only was his explanation non-mechanical in its nature, it seemed impossible to connect it up with any mechanical line of thought. Largely for this reason, it was criticized, attacked, and even ridiculed." Rutherford† wrote: "It is difficult to realize to-day, when the quantum theory is successfully applied in so many fields of science, how strange and almost fantastic this new conception of radiation appeared to many scientific men thirty years ago."

The difficulty of expressing in non-technical language the kind of phenomena dealt with by the quantum theory has preserved it from treatment in the popular press, but the strangeness of some features of the Relativity theory received much popular comment. Some of the ideas which seemed contrary to common sense are expressed in the clever rhymes of two professors of mathematics.‡ For

* *The Mysterious Universe*, 1932 edition, p. 17.

† *Die Naturwissenschaften*, vol. 26, p. 483.

‡ "Relativity Rhymes with a Mathematical Commentary, *Mathematical Gazette*, vol. xi, pp. 22–3. 1922.

example the Restricted Relativity Theory is referred to in Professor Neville's

The Reveller's Charter

"They tell me that Einstein's been thinking
A stick can't be twirled without shrinking,
And we never could know
If our clocks were all slow,
So I needn't say what we've been drinking."

The General Relativity theory, as applied to planetary motion, is dealt with in Professor Piaggio's rhyme:

"If the path of a planet you'd trace,
You've Christoffel's weird symbols to face,
For an orbit, you see,
Is as straight as can be
On a surface in quintuple space."

Still more recently wave mechanics has provided an example of a physical theory which seemed contrary to common sense. "Nothing could have exceeded the apparently wild extravagance of de Broglie's* first work on electron waves which led directly to quantum mechanics."

If attention is confined to theories in physics it is not only in our own time that the postulates of theories have seemed absurd to some even of the leading scientists of the day. A very famous example is that of the concept of action at a distance used in Newton's theory of gravitation. In his celebrated letter to Bentley, Newton himself wrote: "It is inconceivable that inanimate brute matter should, without the mediation of something else which is not material, operate upon and affect other matter, without mutual contact, as it must do if gravitation, in the sense of Epicurus, be essential and inherent in it. And this is the reason why I desire you would not ascribe innate gravity to me. That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance, through a vacuum, without the mediation of anything else by and through which their action may be conveyed from

* R. H. Fowler, *Nature*, vol. 133, p. 855. 1934.

one to another, is to me so great an absurdity that I believe no man, who has in philosophical matters a competent faculty of thinking, can never fall into it."* Huygens said that "Newton's principle of attraction appeared to him absurd." To Benoulli "the two suppositions of an attractive faculty and a perfect void" were "revolting to minds accustomed to receiving no principle in Physics save those which are incontestable and evident." Euler could regard gravitation only as due to some subtle material medium.

In much later times the idea of action at a distance was still regarded as absurd by some physicists. For example E. du Bois-Reymond wrote: "Forces acting through void space are in themselves inconceivable, nay absurd, and have become familiar concepts amongst physicists since Newton's time from a misapprehension of his doctrine and against his express warning."† Balfour Stewart and Tait wrote: "Of course, the assumption of action at a distance may be made to account for anything, but it is impossible (as Newton long ago pointed out in his celebrated letter to Bentley) for anyone who has in philosophical matters a competent faculty of thinking for a moment to admit the possibility of such action."‡

Physics is not the only science using concept which may seem strange or absurd. The more recent theories in psychology may seem strange even to expert psychologists. McDougall, noted for his development of the Instincts Theory of human action, regards as absurd any attempt to use individualistic concepts to explain actions which on the older theories were explained in terms of the concept of altruism.§ Jung regards as "worse than absurd"|| Freud's use of certain sex concepts to explain the behaviour of young children. In this case the objection is perhaps to Freud's use of common terms such as incest in a sense quite different from that in common use. In parenthesis it may

* *Opera*, Horsley's edition, vol. iv, p. 438.

† *Ueber die Greuzen des Naturerkennes*, p. 11.

‡ *The Unseen Universe*, 3rd edition, p. 100.

§ McDougall, *Social Psychology*, p. 70.

|| *Contributions to Analytical Psychology*, English edition, p. 346. 1928.

be noted that Freud's use of common sex words in quite special senses is analogous to the special use of words like 'state' and 'observable' in quantum mechanics. In both subjects a great deal of mental confusion has been produced in the minds of unwary readers.

In this discussion I do not mean to imply that the formulator of a scientific theory tries to use concepts which may appear absurd or contrary to common sense, or that the concepts seem absurd to their originator. I would rather suggest that common sense has nothing whatever to do with scientific theorizing or with the practice of scientific research. Scientists are rare, although the world is full of people well endowed with good sound common sense which they are only too eager to apply on any and every occasion.

To examine how far 'common sense' can help in the practice of research some clear idea must first be got of the meaning of the word. The Oxford Dictionary gives as relevant meanings: "The endowment of natural intelligence possessed by rational beings; ordinary normal or average understanding; the plain wisdom which is every man's inheritance," cautiously adding "(This is 'common sense' at its minimum, without which a man is foolish or insane)." It then goes on, "More emphatically: Good sound practical sense; combined tact and readiness in dealing with the everyday affairs of life; general sagacity," adding as a third set of meanings: "The general sense, feeling, or judgment of mankind, or of a community."

Compare these qualities with those which Faraday regarded as desirable for the practice of scientific research: "The philosopher should be a man willing to listen to every suggestion, but determined to judge for himself. He should not be biassed by appearances, have no favourite hypothesis, be of no school, and in doctrine have no master. He should not be a respector of persons, but of things. Truth should be his primary object. If to these qualities be added industry, he may indeed hope to walk within the veil of the temple of Nature."* Comparing Faraday's statement with the

* Gladstone, *Life*, p. 93. Faraday uses the word 'philosopher' in the sense in which the word 'scientist' would now be used.

Oxford Dictionary's description of the main qualities of common sense, I can see no close similarity.

Moreover, these common-sense qualities in man do not lead to the formulation of atomic, quantum, or relativity theories. Neither do they give us electric power stations, nor radio transmitters, nor X-ray tubes, nor aëroplanes, nor turbines, nor steam engines. They do not show men how to improve the strains of wheat by selective breeding along Mendelian lines, how to make and use artificial manures to increase crops, how to protect the crops from pests, how to make and use machinery for harvesting and transport and preservation of food. They do not show men how to construct the vast water supply and sewerage disposal schemes which under present-day conditions save him from self-destruction by his own filth. Common sense is powerless to make either scientific theories or scientific applications of the facts fitting into the theories. Common sense is in fact a parasite living upon the fruits of uncommon or scientific sense which nowadays does man's hewing of wood and drawing of water, ploughing, sowing, reaping, and mowing.

The idea that common sense has any connection with the practice of scientific research is an idea which does not fit the facts. Common sense is a "prudent safeguard for whoever wants to spare himself the critical study of scientific expressions."* It is "a faculty which, versed in the criticism of the course of events in the outward world, imagines that the very respectable and probable but quite unsystematic maxims there acquired are sufficient to meet all emergencies."† It suffers from three chief defects in being "cocksure, vague, and self-contradictory."‡ Huxley's widely quoted but misleading statement that "Science is, I believe, nothing but *trained and organized common-sense*"§ does not fit the facts in any ordinary meaning of the phrase.

* Enriques, *Problems of Science*, English Translation, p. 329. London, 1924.

† Lotze, *Logic*, English translation, vol. i, p. 248. Oxford, 1888.

‡ Bertrand Russell, *Outline of Philosophy*, pp. 1–2. London, 1927.

§ *Collected Essays*, vol. 3, p. 45.

It can, however, be made to fit by interpreting it to mean that scientific research is done by using qualities of man which are common to all men, though subject to wide quantitative variations in different individuals.

In the textbooks of logic, the device of logical reasoning known as reduction to absurdity, *reductio ad absurdum* or *argumentum ad absurdum* is discussed, and from its name might be thought to be relevant to the present discussion. Examination of the treatment shows that it is a method of presenting a chain of reasonings so that logical inconsistencies such as self-contradiction become apparent. The reasoner does not have to appeal to his sense of humour to apply this device. The appeal is to logical reasoning, not to test by absurdity. The present rejection of absurdity-testing as a tool in scientific theorizing is therefore not concerned with the rejection of any device used in logical reasoning. This particular device of logic bears a name which is apt to be misleading, for everyday observation shows that all men on all occasions do not necessarily regard any kind of logical inconsistency as absurd even in such practical affairs as the political.

(ix) *Diversity.*—When a theory is devised to deal with very diverse and varied facts, usually at least one factor is postulated as subject to *quantitative* variation. Two typical examples of this are the electron theory of atoms and the lattice theory of crystal structure. In the earlier electron theory of atoms it was found that a vast number of physical properties of matter could be explained in terms of an atom postulated as consisting of a small massive nucleus positively charged electrically, round which were one or more negatively charged particles, the electrons. To account for some of the differences in properties of the various chemical elements such as aluminium, iron, and lead it was postulated that the number of electrons and the positive electrical charge on the atomic nucleus was the same for each chemical element but varied for each of the different elements. By this postulation of *quantitative* variation many differences in behaviour could be explained whilst still using the same postulated ideas of positive and negative electricity for substances as diverse as oxygen, sulphur, tin, and lead, to

quote only four examples. The idea of quantitative variation is so widely used in the so-called exact sciences that it is unnecessary to multiply examples of its use in their theories.

The second example, that of the lattice structure of crystals, was carefully chosen so as to include, in readily apparent form, *two* ways of getting diversity. The two ways are, firstly, quantitative variation of factors as in the Kinetic Theory of Gases, and secondly, variation in the arrangement of a given fixed number of factors. This latter method of explaining diversity is used alone in the theory of chemical isomerism. The following discussion is offered especially to those interested in the biological sciences including psychology, and also to those physicists who are apt to think that crystallography is not physics, or that the physicist can get no new viewpoint by studying crystallography.

At the outset it may be emphasized that logically it is quite unnecessary, in order to get variation of a whole, to postulate quantitative variation of one or more of a number of factors which are together to make that whole. This can be illustrated by considering such a simple example as different arrangements of a given number of letters. If the number of letters is n and they are to be placed along a straight line in n fixed positions, then the total number of different arrangements, each consisting of the same n letters, is the product of all the integers from n down to one, decreasing by one at a time. For example, if $n = 5$, then the number of these different arrangements such as A B C D E, A B C E D, etc., is $5 \times 4 \times 3 \times 2 \times 1 = 120$. It can be seen by analogy that without using the idea of quantitative variations and by restricting the number of different qualities to five, represented by the five different letters, 120 different wholes can be formed, solely by the alteration of the arrangements of the separate parts which form the whole.

But this is not the limit of the extent to which variation of a whole can be got without using quantitative variation. In discussing arrangements of letters in space, only the very

simplest arrangement along a straight line has been used. If, however, the same five letters are arranged in two dimensional patterns, as, for example, at the corners of a square with one letter in the middle, then we should have for this one arrangement, 120 more different wholes. By continuing this process and using three dimensional arrangement the number of different wholes would be enormously increased.

These considerations show that without using the idea of *quantitative* variation of factors which differ from each other qualitatively, large numbers of different wholes can be formed solely by using different spatial arrangements.

But now let us go back to the first and simplest arrangement of five letters along a straight line. Suppose we now imagine that each of the five factors represented by the five letters may vary quantitatively. For simplicity suppose that each may have one of two values. How many different wholes can now be made each consisting of five different qualities? In effect we have to deal with ten instead of five factors, but no change will be made in the spatial arrangement considered in the simpler example. It is shown in textbooks of algebra that the number of different wholes, each consisting of five parts, which can now be formed is the number of permutations of ten things taken five at a time. This is given by the product of 10, 9, 8, 7, 6 = 30,240. The number of different wholes has been increased from 120 to 30,240, without making any change in the arrangement of the parts, merely by allowing each of the five different qualities, represented by the five letters, to vary quantitatively to the extent of having two values instead of one. Such a limitation of quantitative variation is very severe and quite unlike the examples seen in concrete objects and events around us. If each of the five factors is allowed to have more than two values and if in addition the different spatial arrangements considered at first are included, then the page will be barely large enough to hold the expression of the number of different wholes which could be formed by the simultaneous use of these five factors. It is evident that quantitative variation of factors may

enormously increase the possibility of explaining different wholes by the use of few postulated qualities.

The lattice theory of crystal structure is an example of the use of both quantitative variation and different spatial arrangements to explain diversity. On this theory many of the differences in the physical behaviour of solid substances are explained in terms of different spatial arrangements of the atoms postulated in the general atomic theory of matter. Many coincidence observations made upon crystals have led to the formulation of certain natural laws of crystallography. In these a large part is played by the concept of symmetry briefly discussed in the chapter on pattern. Symmetry is a pattern property in which the idea of quantity is not essentially involved, and the mathematical theory of the crystal lattice is developed largely by use of the idea of symmetry. The various combinations of about half a dozen elements of symmetry are worked out and it is found that there is a total of 230, the so-called space-groups, which are combinations of elements of symmetry consistent with the experimentally observed facts about crystals. The development of this part of the theory as given in such a book as Hilton's *Mathematical Crystallography* is quite unlike the kind of mathematical reasoning with which the physicist is normally familiar.*

In parenthesis I would add that its study may serve to give the physicist a new viewpoint, revealing what can be done in studying certain natural phenomena by using the idea of order, of pattern properties, quite apart from the much more familiar idea of quantitative variation. It is true that in studying atomic structure the idea of order is also used, for not only is the number of electrons in an atomic model postulated, but also their general arrangement. It so happens that many of the phenomena studied have been explained in terms of very simple arrangements or groupings, and the rigorous symbolical but non-quantitative

* An excellent introduction is chap. iv, pp. 89–112, of Sir William Bragg's *Introduction to Crystal Analysis*, London, 1928; Jaeger's *Principles of Symmetry* discusses many kinds of symmetry shown by both biological and non-biological material.

reasoning used in mathematical crystallography has not been needed. A mere division of the electrons into groups has so far sufficed for dealing with many phenomena.

To return to the discussion of the lattice theory of crystals, we have seen that by using ideas of arrangement or order alone, two hundred and thirty different types of arrangement have been obtained. But there are about half a million different chemical substances known, and the crystals of these occurring naturally or obtained by freezing at very low temperatures have, for each substance, unique properties. The mathematical theory of the crystal lattice gives only two hundred and thirty types of crystal, but observation of actual crystals gives half a million different types. To account for these the idea of quantitative variation is introduced into the lattice theory. One example will suffice to make this clear. If four circles were drawn, one at each of the corners of a square, the resulting figure would have a number of so-called elements of symmetry, such as four planes of symmetry, four twofold, and one fourfold, axes of symmetry. These terms merely express ways in which the figure can be imagined to be moved so that, after movement, it still looks the same from all points of view. But all these same characteristics are still preserved whatever the *size* of the square, so that an infinite number of different figures can be obtained each of which showed the same order. By use of such considerations it is possible to explain the half million different crystals in terms of only two hundred and thirty different kinds of arrangements. Some similarities are explained in terms of order, and some differences in terms of quantitative variation.

From the considerations dealt with in the whole of this section it is evident that diversity can be explained with the aid of few factors *without* the use of quantitative variation, but that the use of quantitative variation enormously increases the power of a theory to account for diversity in the wholes with which the theory deals. The simultaneous use in a theory of the two ideas of order and quantity is an immense advantage when the theory has to deal with much diversity. These results are offered for the considera-

tion of biologists, including psychologists. The kind of diversity with which the physicist and chemist have to deal seems childishly simple compared with the diversity found in living matter. As I see it, the biologist tends to concentrate attention on order, neglecting to some extent the idea of quantity, or perhaps he tends rather to make few statements in his theories about quantitative variation of the concepts used. Particularly in psychology, where the diversity to be explained is so great there seems, except in the theories of Spearman, to be a general omission of explicit statements as to whether or no the concepts used are to be supposed subject to quantitative variation. A typical example is in the Instincts Theory of behaviour where McDougall states that several instincts may be "simultaneously excited; when the several processes blend with various degrees of intimacy."* Such expressions in which there is no definite statement as to whether either or both of the ideas of order and quantity is meant to be used, do not make for clarity of thought in a subject of the very greatest complexity. It will be seen that this suggestion of the desirability of clear statements on both the orders and the quantities postulated in any biological theory is made upon purely formal grounds, not upon a knowledge of the coincidence observations to be fitted into the theories.

(x) *Fit.*—When the mental processes called pure reason are applied to the mental concepts called the postulates of the theory in order to give things expressible as coincidences, then these agree with coincidences derived from observation of concrete objects and events. That is to say, mentally derived coincidences fit those got by the aid of sense organs.

It is not until this stage is reached in theorizing that the scientist can tell whether or no his theory 'works.' When the theoretical results do not fit those got by observation, the original postulates have to be rejected, but it does not follow that the new ones used to take their places are necessarily widely different from the originals. In a mathe-

* *An Introduction to Social Psychology*, 13th edition, pp. 31–2. London, 1918.

matical theory it is often possible to tell from the nature of the logical deduction from the postulates whether the general ideas of the theory are likely to 'work,' even when the first attempt is not successful. The quotations from Einstein's statements given earlier in the previous chapter give illustrations of theorizing where it was necessary to change the general nature of the postulates. Even when mathematics is not used in theorizing, it is sometimes possible to tell from the results of the first attempt whether the postulates must be completely rejected or subject to only slight modification.

It is at this present stage of theorizing that the limitations placed upon the use of the imagination in scientific research become apparent. These limitations can be more clearly perceived in scientific research than in any other human activity. Limitations are placed upon the use or products of the imagination in the arts as much as in the sciences, but the striking difference is in the difficulty of formulating the artistic limitations. The poet may limit himself to the use of, say, blank verse, the painter to a flat canvas and coloured paints, the sculptor to one kind of stone and its inherent physical properties, and the musician to the range and qualities of the various musical instruments or in opera to the limitations of a libretto and the theatre. In all creative use of the imagination there are limitations. In scientific theorizing the fundamental limitation is in one inexorable rule, that the logical derivatives of the postulates, that is of the products of the imagination, shall fit the facts. These facts are, as we have seen, things about which human observers can agree without using lies or threats. There is no necessity to talk about belief or values or epistemology when an astronomer uses various mathematical and physical theories to predict the time and place of an eclipse of the sun. We can each see for ourselves by judging coincidences on the faces of clocks and cross-wires of telescopes.

(xi) *Prediction.*—That characteristic of theories just discussed deals with their use to predict things expressible as coincidence observations, the prediction being got by the use of pure reason applied to postulates. Attention is con-

centrated upon getting such postulates that the theory fits those facts which were chosen before the postulates were devised. The last characteristic to be discussed is found in only a more limited number of theories. It is that they theoretically predict, not only facts known before the theory was formed, but also other types of facts which are only later actually observed for the first time.

Striking examples of this type of theory are found in the subject of Light. For example, Fresnel postulated that light was to be regarded as vibrations occurring in a direction perpendicular to that in which the light travels. On the basis of this Hamilton predicted the occurrence of an optical phenomenon known as conical refraction. That is, a ray of light passing through a certain type of crystal in a certain direction emerges as a conical surface of rays instead of a single ray. This phenomenon had not then been observed, but the prediction was experimentally verified at a later date by Humphry Lloyd of Dublin. Incidentally, this is a good example of a fact obtained by the use of a defective theory, for the Fresnel theory was later discarded.

Another example taken from theories of light is Maxwell's Electro-Magnetic theory. By 1879 a great many facts had been observed in certain types of experimental researches, and these were best explained in terms of the two ideas of electricity and magnetism. Some of the researches starting with Oersted's 1820 experiment dealt with observations where both ideas had to be used. Maxwell expressed in mathematical form some of the ideas of Ampère, Faraday, and Fresnel, relating to this connection between electrical and magnetic phenomena. These together with the use of certain other postulates led to the famous Maxwell Equations of the electromagnetic field. In 1879 the Berlin Academy of Science offered a prize for experimental researches in which these same ideas could be used. This led to the classical experiments of Hertz* which form the

* Hertz gives in his *Electric Waves* (London, 1893) a remarkably clear account, occupying twenty-eight pages, of the origin and development of his researches on the propagation of electric action with finite velocity through space.

basis of radio telephony and broadcasting. Maxwell's electromagnetic field theory is a typical example of the type of theory of which scientists are very fond. Not only did it link together a number of well-known facts hitherto summarized under various natural laws, but its new way of looking at things also suggested types of experiment which lead to new types of facts. Given the theory, the new types of experiment seemed to occur naturally.

To the experimental research worker this last and rare characteristic of scientific theories is one of considerable interest. All theories predict facts and many theories predict particular facts, as, for example, by the substitution of numbers for symbols in mathematical equations. Only few theories have, however, predicted hitherto unsuspected facts. That this latter type of prediction is regarded by research workers as so very desirable illustrates well that research workers regard research as essentially a type of action rather than a type of thinking. The research worker's attitude towards theories is strikingly expressed in the following statement by J. J. Thomson: "The Theory is not an ultimate one; its object is physical rather than metaphysical. From the point of view of the physicist, a theory of matter is a policy rather than a creed. Its object is to connect or co-ordinate apparently diverse phenomena, and, above all, to suggest, stimulate, and direct experiment. It ought to furnish a compass, which if followed, will lead the explorer further and further into unexplored regions. Whether these regions will be barren or fertile, experience alone will decide; but at any rate, one who is guided in this way will travel onward in a definite direction and will not wander aimlessly to and fro."

From much that has been written in these two chapters it might be supposed that a scientist would be satisfied when he had devised a theory to fit certain facts. Whilst he is glad to succeed in this task, he and his fellow-scientists are still more pleased if the theory also suggests other uncompleted tasks. These uncompleted tasks may be either types of hitherto untried experiments, or the application of the concepts of a mathematical theory to explain some pheno-

menon not considered in first formulating the theory. All research workers, including the formulators of theories, like a theory to be both a complete whole or pattern containing the originally selected group of facts, and also an uncompleted pattern revealing gaps to be filled in by new experiments or mathematical work. That these two requirements are not necessarily mutually incompatible can be seen by analogy in visual patterns. That the devising of such theories is rare is a matter of experience.

Let us now consider general prediction by theories. The prediction of new types of coincidence observations is admittedly rare. Is this inherent in theories? As theories become more like the latest theories in mathematical physics, in which the concepts cannot be visualized, will experimental research workers, as devisers of new experiments and selectors of new facts, become unemployed? Will their task be that of actually observing the coincidence observations theoretically derived by the mathematicians? In speculating on this question let us think again of the considerations given in the previous section (x) of this chapter. Let attention be confined to theories in mathematical physics where theorizers are compelled to use clear concepts and where the rigorous use of pure reason is ensured by the wonderful symbolization and technique of mathematics. The conditions here, especially the escape from the ambiguities of words, give the mathematician a very great advantage, and we may reasonably expect that if the development of theories is specially apt to predict new kinds of facts then the aptitude will be shown in these mathematical theories.

To take a specific example let us think of those facts in chemistry for which the chemist uses the remarkable concept of valency. Suppose that those coincidence observations could be logically derived from the logically consistent postulates of some theory in mathematical physics. Would it necessarily follow that they would be so derived by competent mathematicians who were given the postulates but who were not told the facts? This query is not unreasonable because, for example, Maxwell derived facts

without previous knowledge of them. Here is the answer to the specific question, given by Heisenberg: "It seems questionable to me, whether the quantum theory would have found or would have been able to derive the chemical results about valency, if it had not known them before. The method of approximation in the calculations about a molecule . . . is not the usual one, nor the one which looks most natural to the physicist. In the theory of the atom we get the best approximation, in calculating the energy levels, when we first neglect the interaction energy between the electrons and later consider it as a small perturbation compared with the action of the central field. In [the work discussed] however, we consider the interaction of two electrons in the same atom as very large, and take only the interaction between electrons in different atoms as a small perturbation."*

From this statement Heisenberg makes it clear that the mathematical physicist does not proceed upon the lines which he would have followed had he not known about the particular facts. It may be added that this is no question of incompetence or illogical reasoning. Neither is it a question of the common-sense fear or hatred of the new and unusual. Where Heisenberg says that the necessary line was not usual he does not mean that it was contrary to common sense. The line which, without the knowledge of the chemical facts, would have been followed is one which 'worked' in treating of one kind of physical problem, the experimental observations of which were known to the mathematician. This 'usual' or 'natural' treatment is itself a branch of the Quantum Theory using postulates which seemed 'strange and almost fantastic.' Hence the failure without the facts, to adopt the treatment followed with knowledge of the facts, would not be due to fear of the novel.

The consideration of this example may serve to illustrate a feature of theorizing, of which parts of these two chapters

* Given in the discussion on the Structure of Simple Molecules at the British Association Meeting in London in 1931. See *Chemistry at the Centenary Meeting of the British Association*, p. 247. Cambridge, 1932.

may have given a false impression. Although the vital necessity for the use of the imagination in forming postulates was emphasized at the beginning, parts of the later sections may seem to suggest that the elaboration of a theory is almost an automatic process. Some of the statements may give the impression that when the postulates of a theory in mathematical physics have been clearly stated, their originator may as well go away on a holiday leaving a number of mathematicians to complete the theory by working out the logical consequences of the postulates. We have seen, however, that in general this division of labour can reasonably be expected to give the desired results only when these results have been already observed in experiments. Such prediction as that which Maxwell gave seems to depend very much indeed upon the individual. Similar examples are at present too rare to justify any expectation that in the immediate future the main discovery of new types of facts will come from the suggestion of mathematicians.

In the previous four paragraphs attention has been concentrated upon theories in mathematical physics. The structural theory of chemistry may serve to illustrate prediction by postulates which have not been manipulated by the technique of mathematics. In this theory it is postulated that unique chemical and physical properties of substances are to be explained in terms of unique arrangements of the kinds of atoms postulated in the general atomic theory of matter. The ideas are easily manipulated by visualization and have led to the observation of many new facts which, so far as may be seen at present, would not have been discovered without the aid of the theory. In the theory the idea of order plays an important part. The mathematical expression of the theory is, however, far more difficult than that of the lattice theory of crystals, because the chemical theory necessitates the incorporation of the idea of quantity at an early stage to take account of the chemical differences of atoms in terms of different numbers of electrons. The facts so far obtained with the aid of the theory and without mathematical manipulation show that

this latter powerful device is not essential in a theory. As I see it, the chemist by visualization is able to work with the idea of order or arrangement and get qualitative results which help him. Where, however, the reasoning has to be carried out by mathematical devices, the orders required to fit different chemical molecules cannot be expressed in a few simple terms, such as for example by the half-dozen elements of crystallography symmetry, and the mathematician can at present get no results of use to the chemists about the different kinds of order required to elaborate the concept of the different chemical molecules.*

* In Chapters XII and XIII I have tried to analyse the general common features of current scientific theories and theorizing. This has left little room for detailed discussion of particular theories which would serve as illustrations. The earlier chapters of *Domain of Natural Science* (1923) give many examples which would fill this gap. Hobson favours the Description rather than the Absolute theory of science, and the book can be strongly recommended to the general reader wishing for exactly worded statements about famous scientific theories. Although no mathematics is used, the book is not easy reading, on account of the extremely cautious wording of all the statements, as befits the writing of an expert mathematician.

CHAPTER XIV

SOME PROBLEMS OF THEORIZING

A NUMBER of problems of theorizing arise in considering the activity as a whole, or in interpreting the results of completed theoretical treatments. The use of non-metrical mathematics and of purely mathematical ideas which cannot be visualized, have each their own special difficulties of manipulation and of interpretation. Then there is the problem of the relationship of the postulates of a theory to facts. Seven of the eleven characteristics of theories enumerated are quite independent of facts. Moreover, there need be no logical inconsistency in devising theories which are quite independent of facts. A theory rests upon a foundation of *postulates*—on products of the imagination—and not on facts. When facts are taken as the basis of science the logical connection, if any, between postulates and facts must therefore be examined with great care. It is noteworthy that the most widely read general literature of recent scientific research deals almost entirely with the postulated ideas of such theories as have been found to fit facts. The facts themselves are barely mentioned. In the following discussion of some of these problems no special significance is intended in the order of treatment.

MATHEMATICAL TREATMENTS

Two ideas used in theories are those of continuity and discontinuity. Continuity seems especially adapted to theories of elasticity and motion of liquids and light and discontinuity to all kinds of atomic theories from those dealing with the facts of chemical combination and electrolysis, to the kinetic theory of gases and the corpuscular theory of light. There seems to be considerable difficulty in using these two ideas simultaneously in theories. Nevertheless the use of the two ideas seems necessary to link together the facts of, for example, some light and some

electron experiments. It seems necessary to postulate that the concepts of light and of electrons shall each be elaborated so as to include some of the properties of waves and some of the properties of particles. The mathematical treatment as given in the wave mechanics of de Broglie and Schrödinger is of considerable complexity, and suggests that at present mathematics is not well adapted to deal simultaneously with the kinds of order and quantity required. It would appear that all but the simplest pattern concepts need an elaborate development of the mathematics of order. The facts dealt with by wave-mechanics are such that "the picture cannot be broken up into infinitesimally small fractions and the movements of each fraction dealt with independently of the others. On the contrary, according to wave-mechanics, the picture must be held before the eyes as a whole."* My knowledge of pure mathematics is inadequate to estimate how far the study of the combination of complex order and quantitative variation has been already developed. But with the mathematician's interest in general relationships, it must be rather difficult for him to tell what special cases would be of help to research workers, even if of comparatively small interest to the mathematician as such. The following passage by an expert mathematician seems to suggest that at present mathematics is better adapted to deal with quantitative variations than with pattern or order properties: "The fact that mathematical methods are, in a very large class of cases, unable to deal with objects, or with processes, except by breaking them up into parts, and increasing indefinitely the number of those parts, is a significant example of a limitation imposed upon us by what appears to be a definite characteristic of our modes of apprehension. We appear to be unable to grasp some of the relations of a whole, without breaking it up, as it were atomistically, and then proceeding to reconstruct the whole by a synthetic process . . . which by its very nature is such that the whole is never actually reached within the process."†

Many writers have omitted the pattern concept from their

* Planck, *Where is Science Going?* p. 63. 1933.

† Hobson, *The Domain of Natural Science*, p. 121. Cambridge, 1923.

analyses of research technique and have regarded scientific knowledge as essentially metrical knowledge. In 1874 at the time of the founding of the Cavendish Laboratory in Cambridge, Maxwell wrote, referring to the subject of physics: "In every experiment we have first to make our senses familiar with the phenomenon, but we must not stop here, we must find out which of its features are capable of measurement, and what measurements are required in order to make a complete specification of the phenomenon."* "In experimental researches strictly so-called, the ultimate object is to measure something which we have already seen—to obtain a numerical estimate of some magnitude." In his Cambridge Lecture of October 1871, he refers to the "characteristics of modern experiments—that they consist principally of measurements," and explains that "the opinion seems to have got abroad that, in a few years" (i.e. after about 1875) "all the great physical constants will have been approximately estimated, and that the only occupation which will then be left to men of science will be to carry these measurements to another place of decimals." However he warned his hearers that: "The history of science shows that even during that phase of her progress in which she devotes herself to improving the accuracy of the numerical measurement of quantities with which she has long been familiar, she is preparing the materials for the subjugation of new regions, which would have remained unknown if she had been contented with the rough methods of her early pioneers."

The view that science is essentially metrical knowledge is still advanced. Lodge in his British Association address of 1913 states: "Science is systematized and *metrical* knowledge and in regions where measurement cannot be applied it has small scope." So clear a thinker as the late Lord Balfour had evidently gathered this impression that scientific method is essentially measurement, for in his address given in 1917 at the opening of a new wing at the National Physical Laboratory, he said: "Science depends upon measurement, and things not measurable are therefore

* *Collected Works*, vol. ii, p. 244.

excluded, or tend to be excluded, from its attention." We have seen, however, that pattern properties are not measurable in the sense that they depend upon the order rather than upon the quantity idea. Even in physics where measurement plays so large a part, the new wave mechanics is found necessary to deal with facts which cannot be explained in terms of measurable quantity alone. The insistence upon the metrical aspects of science would appear to be due to the fact that the greatest advances have been made where mathematics has been applied. Metrical aspects of things seem easier to grasp even in mathematics, so that metrical mathematics is more fully developed than is non-metrical. It follows that applications of non-metrical mathematics capable of dealing with pattern properties have to wait for the development of the necessary pure mathematics.

It is becoming apparent that the metrical aspects alone of all subjects cannot express all of the things in which scientists, as such, are interested, and the development of the mathematical theory of relationships other than metrical will be a great help to scientific research when it has to face either problems of greater complexity in inorganic nature, or any problems in organic nature. In the most recent work in physics as in the wave mechanics or in biological problems (including the unsolved problems of civilization) both the whole and the parts must be continually kept in mind. The metre rule is no longer the physicist's magic wand, alone capable of dealing with all problems of physics, and outside the laboratory the ourselves-alone idea of, for example, intense nationalism, in practice does not work. Both inside the laboratory and outside, man is meeting problems needing 'contrapuntal' thinking, or, to vary the metaphor, needing the type of mental activity usually associated with the artist who can pay infinite attention to detail without losing sight of the whole.

THEORIES ARE PRODUCTS OF INDIVIDUALS

The previous comments suggest that theorizing with the aid of mathematics is getting no simpler to man with the

accumulated knowledge of mankind. Could this difficulty be got over by two or more men putting their heads together to formulate a theory? There is much talk nowadays about team work in research. It is said that the day of the individual in research is over or almost over. Although I personally would explain this statement in terms of concepts well known to the psychoanalysts, there is one thing of which we can be sure with man as at present constituted. Although many heads and many hands can help in many research activities, there is one activity which must be done by the individual alone. That activity is the origination of new ideas. The postulates of theories invariably come out of individual minds, not out of mobs, herds, teams, or conferences. As Huggins put it in his presidential address at the Royal Society Anniversary Meeting on November 30, 1905: "From individual minds are born all great discoveries and revolutions of thought. New ideas may be in the air, and more or less present in many minds, but it is always an individual who at the last takes the creative step and enriches mankind with the living germ-thought of a new era of opinion."

Devisers of theories have to make themselves thoroughly familiar with all the facts to be covered by the theories, and these facts have usually been discovered by others. Devisers of theories, too, benefit by the accumulated knowledge of mankind, but in the end all the knowledge has to go through a single human mind in order to produce a theory. By all tugging at the same rope men can add together their muscular powers, but there is known at present no method by which they can add together their mental powers in at all the same kind of way. Muscles can be added, minds cannot. One generation inherits from the previous generations, not only facts but also ways of looking at facts, and this latter inheritance may sometimes be an encumbrance in the integration of the ever-increasing bulk of facts. All the evidence seems to suggest that theory making is an intensely individualistic human action of which the products may at first seem very strange or fantastic to even some scientists. For this reason I doubt the psychologically

comforting notion that, for example, each physicist is a potential Faraday or each mathematical physicist a potential Einstein. Galton wrote: "When apples are ripe, a trifling event suffices to decide which of them shall first drop off its stick; so a small accident will often determine the scientific man who shall first make and publish a new discovery."* A similar undue emphasis of the influence of external environment upon human action at the expense of the equally relevant factor, biological constitution, is expressed in the latter part of the following passage: "Every discovery, however important and apparently epoch-making, is but the natural and inevitable outcome of a vast mass of work, involving many failures, by a host of different observers, so that if it is not made by Brown this year it will fall into the lap of Jones, or Jones and Robinson simultaneously, next year or the year after."† The technique of experiment is not sufficiently developed to enable us to tell whether these statements fit the facts or whether the statements could not better be interpreted in terms of the idea of conservation of superiority already used elsewhere in this book. If the statements are accepted we must suppose that a man like Faraday not only worked but also slept under the Trees of Knowledge, as he seemed to be always there when the fruits were falling.

THE WORKING HYPOTHESIS

For clearness a note may be added on the use of the term 'working hypothesis.' Although there is no generally observed rule about the use of the terms 'theory' and 'hypothesis,' the term 'working hypothesis' is often used to denote a simple hypothesis or theory which is used as the basis of a discussion of some facts or as the basis of some experiments. For example, if a bacteriologist started with the idea that some disease was always associated with the presence in the sufferer of a certain kind of bacillus which had not

* *Nature*, vol. 118, p. 686.

† E. H. Starling, "Discovery and Research," *Nature*, vol. 113, pp. 606–7. 1924.

at the time been observed, this supposition might be called a working hypothesis. If later he applied the laboratory technique of bacteriology and was able to isolate as a stained preparation a new kind of bacillus, he would be said to have verified his working hypothesis.

In the sense in which the word 'postulate' is used in this book a postulate is something which cannot be, or at least has not been, verified by coincidence observation. In this simple example it would appear that the hypothesis is so simple that there is no need to write down postulates and logically deduce from them things which can be observed by judgment of coincidence. The working hypothesis at once implies that stained bacilli of shape different from those already known may be observed. If the research worker does later observe them, the working hypothesis is said to be verified. What is meant when an elaborate theory is said to be verified?

VERIFICATION

It is evident that if the *postulates* of a theory were themselves verified by coincidence observation they would at once become facts and the theory would lapse. For example if the atom were observed by judgment of coincidence we could no longer speak of an atomic theory. The atom would change from a mental concept to a fact. An atom could not be postulated if it had already been observed by the coincidence method.

On this pattern theory of scientific theorizing it becomes meaningless to ask if a theory or hypothesis is true. One can ask only if the theory is logically sound, and if the theoretically derived coincidence observations (which are not necessarily quantitative measurements) agree with those derived by the use of sense organs. Briefly, one can only ask if the theory fits the facts and *no theory can be claimed to be the only theory which could fit the same facts.* As Whitehead, using the term 'scientific doctrine,' puts it: "Only one question is asked: Has the doctrine a precise application to a variety of particular circumstances so as to determine

the exact phenomena which should then be observed? In the absence of these applications, beauty, generality, and even truth, will not save a doctrine from neglect in scientific thought."*

It is representative of the practice of most laboratory research workers, to say that a theory has been verified when it has been shown to fit the facts. Only on the Absolute theory may one add 'therefore the theory represents a piece of Absolute Truth.' Research workers do not like the phrase added at the end of a piece of research and no research journal would, I think, publish such an addition. It appears only in some popular scientific and philosophical writing. The main objection of research workers to the statement is that no chain of logical reasoning can be put between it and the preceding statement about the theory fitting the facts and no tests have so far been given which can be applied to identify Absolute Truth. The research worker is nervous "Lest he become deceived." As James put it: "Science has organized this nervousness into a regular *technique*, her so-called method of verification; and she has fallen so deeply in love with the method that one may even say she has ceased to care for truth by itself at all. It is only truth as technically verified that interests her. The truth of truths might come in merely affirmative form, and she would decline to touch it."† Research workers as such are not concerned with what things might conceivably be. They concern themselves solely with what can be observed, using coincidence observations for testing or 'verification,' and using internal observation of abstract and other mental concepts, including theories, to deal with the sense data of coincidence observations.

REALITY

When a theory has been verified in the sense just explained, does that mean that such postulated concepts as atoms, electrons, ether, gravitation, magnetism, energy, or entropy

* *Principle of Relativity*, p. 3.

† *The Will to Believe*, p. 21. London, 1897.

which may have been used in the theory, are real? Of all the words that are so vague, there are few so vague as *real*. Before the question can be answered we must try to get a clear idea of what men mean by real. It will help towards clarification to note also with what the real is contrasted.

Metaphysicians contrast reality with appearance. They define reality as the inner being or ultimate nature of things, and contrast this with more superficial characteristics which things show when we have imperfect or incomplete knowledge of them. This seems to be the sense in which the word is often used by scientists. For example, Lodge writing of Einstein's relativity theory says: "In such a system there is no need for 'Reality:' only phenomena can be observed or verified: absolute fact is inaccessible. We have no criterion for truth; all appearances are equally valid; physical explanations are neither forthcoming nor required. . . . Matter is, indeed, a mentally constructed illusion generated by local peculiarities of space." These are views with which he does not agree and he goes on to say: "Notwithstanding any temptation to idolatry, a physicist is bound in the long run to return to his right mind; he must cease to be influenced unduly by superficial appearances . . . geometrical devices and weirdly ingenious modes of expression; and must remember that his real aim and object is absolute truth . . . that his function is to discover rather than to create, and that beneath and above and around all appearances there exists a universe of full-bodied, concrete, absolute Reality."* In these statements reality is contrasted with 'superficial' appearances. A similar use of the term seems to be implied in the following more cautious statement by Soddy: "The modern habit of thought recognizes things as having a real existence apart altogether from the particular qualities or properties by means of which the things make themselves known to the five senses. The acceptance of this habit of thought among scientific men has been due mainly . . . to its fertility. . . . Deep down somewhere in the processes of thought the ultimate test of reality appears to be the Law of Conservation. . . . Is matter real or a mere

* *Nature*, vol. 106, p. 796.

impression of the mind?"* It will be noted that here real is contrasted with 'mere' impression of the mind and the test of reality appears to be a test of what is conserved. Another view is that real means what can be observed by the use of the sense organs, in making coincidence observations. It is on this view, according to which pain, for example, is not real, that the poet, referring to a gentleman from Deal, wrote:

"Although I'm told pain isn't *real*
If I sit on a pin
And it punctures my skin
I dislike what I fancy I feel."

In contrast with these views where 'mere' impressions of the mind are presumably regarded as unreal, the opposite view may be noted that mental concepts express ultimate reality. Referring to the wave system of an electron, Jeans† writes: "It exists in a mathematical formula; this, and nothing else, expresses the ultimate reality." Speaking more generally he says: "The universe can be best pictured, although still very imperfectly and inadequately, as consisting of pure thought, the thought of wnat, for want of a wider word, we must describe as a mathematical thinker."

These are only a few of the views on what is real. They suffice to show that in the present state of the literature no general agreement has been reached as to the scientist's use of the term. Equally eminent writers not only give different definitions but some precisely invert the meaning of others. If only different definitions were found then it would be reasonable to try to see if each of the definitions was emphasizing some different property or aspect of reality. When, however, one authority says that 'real' means impressions of the mind and another authority says that 'real' means not impressions of the mind, we are face to face with a logical inconsistency. 'Real' cannot both be and not be impressions of the mind. Under these conditions I fail to see how any answer can be given to such a specific question

* *Matter and Energy*, p. 40.

† *The Mysterious Universe*, p. 142 and p. 136.

as 'Are electrons real?' One can only ask 'What do you mean by real?' or 'What tests must be applied in order to decide whether a thing is real or not?' Research workers are concerned with what they can observe about things, not with what things are.

Nevertheless, Max Planck, one of the greatest physicists of the day, wrote in 1913: "Whoever rejects faith in the reality of atoms and electrons or the electro-magnetic nature of light waves, or the identity of heat and motion, cannot be found guilty of a logical or empirical contradiction; but he will find it difficult from his standpoint to advance physical knowledge." But does not this passage use as established a certain unestablished theory of human action? To "advance physical knowledge" a man must carry out certain observable actions such as, for example, publishing a scientific paper. This man is an animal with certain external and internal environment and some results of his actions can be observed in his published research papers. To insist that he could or did not carry out those actions, or that he found it more difficult to do so unless he had faith, is to insist that the concepts of faith or belief must be used in any theory of human action which fits the observed facts of human action. As I understand it belief or faith is not something directly experienced by an individual, as is, for example, pain. It is rather a mental concept useful to explain human actions, especially when it is postulated that the use of pure reason is to be regarded as basic. In trying to express by what means a man's faith could be tested, we should, I think, end by discussing his observable actions.

To end this brief discussion of a highly controversial subject I add the following result and comment which must not be taken as necessarily representative of the views of scientists in general. The result is, that in every verbal or written discussion of real which I have heard or seen, that which is called unreal is invariably referred to as 'mere,' 'only,' 'superficial,' 'empirical,' and like terms which express that the unreal is assessed as lower in value than the real. This suggests that although no uniformity of treatment can

be seen when reality is regarded as an intellectual or phil sophical problem, all the treatments show a biological psychological uniformity.

Men as creatures of pure reason reach logically inco sistent results, but men as biological creatures all agree th the real is better than, or superior to the unreal. In practi then, the word 'real' is used by scientists and philosoph alike to express an emotional attitude and not to expr an observable property of the thing which is called re To end with our specific question 'Is an electron rea It may be said, firstly, that so far scientists have not reach agreement as to what they mean by real. Secondly, the is no published research paper dealing with electrons which either a mathematician or a physicist would ha to alter anything in his equations or his experiments becau he used the additional idea that electrons were real unreal. The whole of electron physics both theoretical a experimental is quite independent of the inclusion omission of the word 'real.'*

Writing on the use of words whose sense is not defin Heisenberg says: "In this connection one should particula remember that the human language permits the constru tion of sentences which do not involve any consequen and which therefore have no content at all—in spite of t fact that these sentences produce some kind of picture our imagination; e.g. the statement that besides our wo there exists another world with which any connection impossible in principle, does not lead to any experimen consequences, but does produce a kind of picture in t mind. Obviously such a statement can neither be prov nor disproved. One should be especially careful in usi the words 'reality,' 'actually,' etc., since these words oft lead to statements of the type just mentioned."† This sta ment is thoroughly representative of the scientist's lack interest in concepts which, even though they be clear enou

* The mathematical use of the words 'real' and 'imaginary' technical and the words 'laer' and 'yranigami' would serve equ well in this technical use.

† *The Physical Principles of the Quantum Theory*, p. 15. Chicago, 193

to 'produce some kind of picture in our imagination,' do 'not lead to any experimental consequences' or cannot be 'technically verified.' If such words as 'reality' and 'existence' were banished entirely from scientific literature, any tears shed by research workers would be tears of joy, not of sorrow.*

IS EVERYDAY OBSERVATION A KIND OF THEORIZING?

Now that both observation and theorizing have been discussed, a brief re-examination may be made of the question 'Are atoms and such familiar objects as chairs essentially the same, differing only in, say, size and shape?' In view of what has just been said about the restriction of the scientist's interests to what can be technically verified, it is evident that the only part of the question which can be dealt with scientifically relates to what can be subject to tests of observation and experiment. These reveal a very striking difference. When coincidence observations of a chair are made, the object called the chair forms one of the two things essential for judgments of coincidence. As no man has yet claimed to have observed an atom by the coincidence method, we have as a definite result that a chair can be observed by the only method in which men reach agreement, whilst so far an atom has not been so observed.

It is claimed by some writers that even when the coincidence method is directly applicable, as in observing a chair, some kind of synthesizing process analogous to theorizing has to be used in order to get any idea of the whole which is called the chair. On this view common observation is regarded as a process of rapid synthesis, a rapid building up into a whole, of a number of separate details which are first seen in isolation or unrelated; one first sees the four legs, seat, and back, and then rapidly synthesizes them into the whole visual impression of the chair. Alternatively on

* For analysis, an excellent example in mathematical physics is the two papers by Einstein, Podolsky and Rosen, "Can Quantum Mechanical Description of Physical Reality be Considered Complete?" *Physical Review*, 47, p. 777. 1935; and Bohr, 48, p. 696. 1935.

the Pattern theory, observation is to be regarded as a process of rapid analysis in which the whole is seen first, and is then analysed into smaller wholes as observation is continued. On this view a visual impression of the chair as a whole is first seen, and is then analysed into such smaller wholes as back, seat, and group of four legs. This analysis view seems to fit better the facts of observation given in Chapters VII and VIII, and to imply that sense data do not come singly. Even in the very simple form of a coincidence observation we are dealing not with an isolated thing but with a whole analysed into two objects. If we took away one of the two objects we should still be left with the other *and* the contrasting background without which it would not be seen at all.

Before accepting the pattern theory of observation it may be objected that our impression of a chair is much more elaborate than that of a mere visual object. But note how data derived from senses other than vision are presented as wholes together with the visual data. For example, in taking hold of and sitting in a dining-chair, on pulling it to the table, we use touch, hearing and vision all at once, and it is only in later stages, and with repetitions, that these separate impressions are analysed out. In both the Pattern and the Synthesis hypotheses of observation, the concept of memory is used. There would therefore appear to be no logical necessity to suppose that the elaborate conception got in adult life of such a familiar object as a chair, incorporating impressions of seeing, hearing, touching, and smelling, is essentially the product of a kind of theorizing. Sense data are first taken in as wholes even when two or more of the senses are in active use at the same time. Man appears to be so made that he is first aware through his sense organs of his environment as a whole without any conscious synthesizing process. In forming scientific theories he seems on the contrary to be very much aware of a synthesizing process.

Returning then to our question 'Are atoms and such familiar objects as chairs essentially the same, differing only in, say, size and shape?' we may answer that the scientist

as such is interested only in 'truth as technically verified.' He is concerned not with what things are, but with what tests he can apply, using his sense organs, and with mental concepts into which the sense data can be fitted. As to tests, the thing called a chair can directly give sense data; it can form one of the two parts essential for judgment of coincidence. The sense data got from observation of a chair can be got by using the type of human judgment in which agreement between observers is reached. Sense data can also be got from the chair, in the form of a whole, by using common observation. In contrast with this, the thing called an atom has so far yielded neither types of sense data. It may therefore be said that in considering tests, the atoms and the chair show differences rather than similarities.

As to the second point in which the scientist as such is interested, 'the mental concepts into which the sense data can be fitted.' It will be admitted that we cannot manipulate with our minds the chairs and atoms which we are said to manipulate with our hands. Ideas or concepts are formed for manipulation by the mind. Can the same kind of mental manipulation be used in dealing with our ideas of a chair and of an atom? In some circumstances the idea of a chair and of an atom can be treated in the same way. For example, Newtonian mechanics can be applied, and can predict coincidence observations which are later technically verified. There are, however, circumstances such as those studied in quantum physics, in which the kind of mental manipulation found suitable for dealing with an object such as a chair gives facts which are not technically verified when applied to an atom. Hence, in this respect, although the atom and the chair can sometimes be treated similarly, there are other occasions when they must be treated differently.

Different tests must always be used, and different ways of thinking must sometimes be used in dealing with an atom and a chair. It is noteworthy that the test which is never applicable in dealing with an atom, is the most reliable means so far discovered of reaching agreement between different observers.

THEORIES AND CAUSES

So far little has been said about causes. In scientific literature the word cause is used in a number of different senses and in the sense in which it is most used by research workers nowadays, it refers to a relationship which can be established solely by the use of experiment. In this sense it will be discussed in connection with the technique of experiment in the following chapter. In the present section the word cause is used to mean something which makes a phenomenon occur or more colloquially 'something which gets inside and makes the wheels go round,' and we will ask 'Are scientific theories guesses at causes?' or 'Do scientific theories tell us why things happen?' To this question I would answer 'No.' Scientists have ceased to be interested in asking 'Why?' and have changed the question to 'How?' Scientific research gives description, not causes. Using the idea of pattern completion in human action, it will be seen that causal relationships of the 'Why' type are always uncompleted patterns or wholes. No matter how elaborately a phenomenon is analysed into causes a completed whole is never obtained, for once a cause is regarded as telling why something happens, we still have to find a causal relationship for even the most far removed cause from the phenomenon studied. Once we begin to ask, 'Why?' there is, from the very nature of the question, no end to the inquiry. This may stimulate research, but is not the only stimulus to research.

The difference between the 'causal' and the 'descriptive' view of scientific theories can be illustrated by an example. On the 'descriptive' view, gravitation is a mental concept which makes a pattern or completed whole of a vast number of coincidence observations made upon falling apples, tides, and planets, to mention only three types. Gravitation does not tell us why apples fall, but how they fall. To ask for a theory of gravitation itself, is to ask for a theory of a mental concept so that we should ask not a physicist but a biologist or a psychologist. Einstein's General Relativity theory is not a theory of gravitation. It is a physical theory

dealing with the same coincidence observations, amongst others, as are dealt with also by the alternative concept of gravitation.

On the 'causal' view of scientific theories, gravitation is regarded as something which, for example, makes apples fall and tides rise and fall. The idea is so clear and useful that it is felt that gravitation must be awarded, *honoris causa*, some at least of the properties of concrete objects. It therefore becomes reasonable to ask what physical agencies cause gravitation. The attempts to formulate a physical theory of gravitation itself may be regarded as triumphs of muscle over mind. The concept of gravitation is used to deal with certain movements of actual objects. Let us therefore note how in daily life we ourselves move non-living objects. We get behind and push or get in front and pull or we can throw stones at them. The boy who accidentally gets his ball into a pond too far from the edge to recover with the aid of a stick, can throw stones to the far side of the pond, and, with the aid of the waves set up, he can gradually get the ball to come near enough to be reached.

The ideas of every one of these muscular ways of making objects move has been used in some one or other of the numerous unsuccessful attempts to formulate a physical theory of gravitation itself. Le Sage's, the least unsuccessful of the theories, uses the idea of moving objects by throwing stones at them, the stones being 'ultramundane corpuscles' which were supposed to be so small that they seldom collided with each other. When two gravitating objects moved towards each other, it was pictured that each partly screened the other from the general bombardment of the ultramundane corpuscles filling all space, the excess of impacts on the two outer surfaces serving to push the two objects towards each other. Maxwell calculated that the energy of the corpuscles spent in maintaining the gravitation of a pound of matter towards the earth was millions of millions of foot-pounds per second. He also showed that if the corpuscles rebounded from gravitating objects with the same velocity as that with which they struck, then there would be no excess of impacts on one or other side and the gravitating effect would not

be observed. If they rebounded with a lower velocity there was no postulate to deal with the energy lost. If it was supposed that any appreciable part of the excess appeared as heat, then the gravitating bodies of the universe would be raised to a white heat in a few seconds.

As I see it, this latter result may be used metaphorically, and I would say that a man who tries to formulate a physical theory of gravitation itself is merely making it hot for himself. There is little doubt that in the future men will devise theories which link up the coincidence observations of Newton's gravitation phenomena with other types of coincidence observations. Einstein's general theory does this. But that is apparently not what Le Sage and similar workers were trying to do. They were trying to explain not the phenomena of gravitation but gravitation itself.

Einstein has shown that all the phenomena commonly called gravitation phenomena can be explained alternatively without using the idea of gravitation. If gravitation be regarded as a mental concept, then the only conceivable way at present of giving a purely physical explanation of gravitation itself would be by formulating a purely physical theory (containing no purely biological or psychological concepts) to explain how it came about that a certain highly remarkable animal reacted so as to produce that remarkable set of 'overt responses' called the writing of the *Principia*. Then, a physical theory of gravitation itself would be given. Newton did not discover 'gravitation,' he created it.

CHAPTER XV

SOME FACTORS IN EXPERIMENTAL TECHNIQUE

ONE necessity for the development of the technique of experiment is apparent from the facts of human observation. From some of these facts given in previous chapters it is evident that human observation under the conditions of everyday life is frequently wholly unreliable. The same person may report contradictory evidence if he sees a phenomenon under ordinary conditions and then again under the controlled conditions of laboratory observation where the coincidence method can be used. In the laboratory we can often arrange to see the same phenomena repeated many times, at expected times, so that we are not taken by surprise, and instruments can often be used to aid observation. In considering the contradictory evidence the scientist uses that obtained from the controlled observation of experiment. One use of experiment is then to give mutually consistent observations.

But have men always wanted reliable observations? Must the technique of experiment, which is admittedly a means of getting reliable observations, be regarded as means which men continually strived to evolve? To answer this question it may be recalled that some experiments are so simple that they can be done in the home with materials which have been available to some men for many hundreds of years. Think of Pliny's statement that the blood of a goat would shatter a diamond to pieces. Comparatively recently we find Legh writing, "The Diamonde, which neither iron not fier will daunt, the bloud of the gote softneth to the breaking." Still more recently even Browne wrote, "We here it in every mouth, and in many good Authors reade it, that a Diamond, which is the hardest of stones, not yielding unto Steele, Emery, or any other thing, is yet made soft and broke by the bloud of a Goat." This false observation could have been tested long before the days of laboratories. In classical literature many hundreds of similar examples can be found

of false observations which could have been tested at the time the statements were first made. Apparently the men who left records were not waiting for experiment to give them a means of making reliable observations, for they did not use the adequate means which they already had.

It may next be asked if men have changed in this respect. Do we nowadays want reliable observation in everyday life? Suppose I were driving a motor-car which became involved in a road accident at a time and place where a cinema film was being taken. There would be a reliable source of coincidence observations good enough to please any scientist. Although I am a scientist with a passion for coincidence observations, do you think that I should therefore welcome this particular set? Of course, I should welcome them if they showed that I was not at fault, but I should very definitely not welcome them if they showed me to be at fault. There is no need to labour this point that in everyday life reliable observations may or may not even nowadays be wanted according to circumstances. Now in scientific research reliable concordant observations are very definitely sought always and in all circumstances. One necessity for the use of experiment is then to get reliable observations and the *wanting* of reliable observations seems to be at the basis of experimental technique.

"I DETERMINED, HOWEVER, TO TEST THE MATTER FOR MYSELF"

The appeal to experiment seems to be a matter of temperament rather than of technique. It may well be that both before and after the time of Galileo some men have been content to reason about things and others to compare the results of their reasoning with those of observation. Experiment is used also in childhood. The necessity for laboratories and complicated apparatus in much current research is apt to hide the basic idea of the appeal to experiment. As discussion of the specialized experimental techniques of the individual sciences is outside the scope of this treatment of scientific action, an example will be

taken from everyday life. The experimenter was a child about six years old and the account, written by him in adult life, is given in Edmund Gosse's *Father and Son. A Study of Two Temperaments.* (1907.) His father, P. H. Gosse, F.R.S., was a distinguished naturalist.

"The question of the efficacy of prayer, which has puzzled wiser heads than mine was, began to trouble me. It was insisted on in our household that if anything was desired, you should not, as my Mother said, 'lose any time in seeking for it, but ask God to guide you to it.' . . . My parents said: 'Whatever you need, tell Him and He will grant it, if it is His will.' Very well; I had need of a large painted humming-top which I had seen in a shop-window in the Caledonian Road. Accordingly, I introduced a supplication for this object into my evening prayer, carefully adding the words: 'If it is Thy will.' This, I recollect, placed my Mother in a dilemma, and she consulted my Father. Taken, I suppose, at a disadvantage, my Father told me I must not pray for 'things like that.' To which I answered by another query, 'Why?' And I added that he said we ought to pray for things we needed, and that I needed the humming-top a great deal more than I did the conversion of the heathen or the restitution of Jerusalem to the Jews, two objects of my nightly supplication which left me very cold." Gosse records that his Mother was much baffled by the logic of the argument and that his Father was much annoyed by it. The Father forbade the child to pray for things like humming-tops. "His authority, of course, was paramount, and I yielded; but my faith in the efficacy of prayer was a good deal shaken. The fatal suspicion had crossed my mind that the reason why I was not to pray for the top was because it was too expensive for my parents to buy, that being the usual excuse for not getting things I wished for."

In applying the technique of experiment to the study of the efficacy of prayer this child had chosen to rely upon coincidence observations. Whether or no he got a certain painted humming-top was a matter of judgments of coincidence which would have presented no difficulty at all.

But the child seemed to realize that the parents' financial

conditions might account for his not getting the top. In this simple experiment, there was a factor which might account for a negative result, but which was beyond the experimenter's control. Gosse records that he continued to be greatly puzzled and his mind continued to dwell on the mysterious question of prayer. About this time he was taken to a missionary service and his thoughts were turned to the subject of idolatry, which had been severely censured at the meeting. He writes, "I cross-examined my Father very closely as to the nature of this sin, and pinned him down to the categorical statement that the idolatry consisted in praying to anyone or anything but God himself. Wood and stone, in the words of the hymn, were peculiarly liable to be bowed down to by the heathens in their blindness. I pressed my Father further on this subject, and he assured me that God would be very angry, and would signify his anger if anyone in a Christian country, bowed down to wood and stone. I cannot recall why I was so pertinacious on this subject, but I remember that my Father became a little restive under my cross-examination."

Here follows a typical use of scientific action. "I determined, however, to test the matter for myself, and one morning, when both my parents were safely out of the house, I prepared for the great act of heresy. I was in the morning-room on the ground-floor, where, with much labour, I hoisted a small chair on to the table close to the window. My heart was now beating as if it would leap out of my side, but I pursued my experiment. I knelt down on the carpet in front of the table, and, looking up, I said my daily prayer in a loud voice, only substituting the address, 'O Chair!' for the habitual one.

"Having carried this act of idolatry safely through, I waited to see what would happen. It was a fine day, and I gazed up at the slip of white sky above the houses opposite, and expected something to appear in it. God would certainly exhibit his anger in some terrible form, and would chastise my impious and wilful action. I was much alarmed but still more excited; I breathed the high, sharp air of defiance. But nothing happened; there was not a cloud

[*sic*] in the sky, not an unusual sound in the street. Presently I was quite sure that nothing would happen. I had committed idolatry, flagrantly and deliberately and God did not care."

The interpretation of the result is masterly. Here is his comment. "The result of this ridiculous act was not to make me question the existence and power of God; those were forces which I did not dream of ignoring. But what it did was to lessen still further my confidence in my Father's knowledge of the Divine mind. My Father had said, positively, that if I worshipped a thing made of wood, God would manifest his anger. I had then worshipped a chair, made (or partly made) of wood, and God had made no sign whatever. My Father, therefore, was not really acquainted with the Divine practice in cases of idolatry."

The two experiments here cited illustrate very strikingly several characteristics of scientific action. On being told of something which could be tested "I determined . . . to test the matter for myself." Wherever possible Faraday repeated any experiment of which he had heard and he often asked to be allowed to handle the apparatus himself when he was shown an experiment. Unfortunately many current experiments are too complicated and expensive to repeat, but even so, such experiments as X-ray and electron diffraction by crystals were repeated by others soon after the original accounts were published.

The necessity for controlling any factors which might affect the observed results of an experiment render the humming-top experiment less satisfactory than the idolatry experiment, as Gosse himself saw. The child's close cross-questioning of his father before devising the details of his experiments is characteristically scientific, for experimenters do not work blindly. In conventional language they may be said to start, as did Gosse, with some aim or purpose clearly before them. As often as not they fail to find the thing sought, but in seeking it they do find something. Caution in interpretation is strikingly shown by Gosse's supplementary hypothesis that his father might not be a reliable guide to the actions or psychology of the Almighty

and that this hypothesis also would fit the result of the chair experiment.

MEASUREMENT

Measurement is one widely used device in the technique of experiment. The basic ideas and their relationship to judgment of coincidences and of betweenness can be seen in the measurement of length. It is customary to discuss this measurement in terms of the Absolute theory of science. Such an object as a steel sphere would be regarded as having, under prescribed conditions, a certain absolute length. It would be necessary to consider how the process of measurement affected this length. In using a micrometer screw gauge slight pressure would be applied at the two ends of a diameter of the sphere held between two metal jaws. This pressure would then be supposed to alter the real length of the object. *On the Absolute theory of science*, all processes of measurement alter that which is measured. Measurement of the length of an object is so defined that the application of the process cannot give that length, the *real* length, in terms of which the measurement of length is defined. This was generally recognized in the discussion of laboratory measurements. A similar kind of Absolute definition of measurement has been used in mathematical physics and has led to the Principle of Indeterminacy.

The patternist has to approach the problem of measurement from a quite different viewpoint. He is here, as elsewhere, unwilling to make any absolute statements about what can or cannot be done. He will say what *has* or *has not* been done, and he may base his action upon these statements, without speaking of possibility or of probability. He rejects as unscientific any unqualified absolute statement about what can or cannot be done. Next, he separates his physics from metaphysics. To speak of the real, true, or absolute length of an object is to use metaphysical terms. The use of metaphysical terms may or may not be accompanied by the use of metaphysical ideas. The one is unscientific and the other unnecessary. The patternist's definition of measurement must be made in the language of

action rather than in that of inference, and the idea of agreement between human observers rather than that of philosophical truth must be used as basic. In the following paragraph an attempt is made to give an outline of such a definition. The processes are carried out in such institutions as the National Physical Laboratory at Teddington and the details are published in the technical literature of metrology.

It is found that two blocks of metal can be cut into such shape that each edge or face of each block can be made to coincide in turn with each edge or face of the other block. Their coincidence can be judged by sight or by touch. A piece of metal can also be cut so that the one edge coincides with a fine suspended wire carrying a lump of metal at the unsuspended end. The coinciding edge can be called a 'straight edge' and the two blocks of metal 'cubes with plane faces.' It is found that if the two cubes are put together with one face and its edges coinciding, that the straight edge can also be made to coincide with any of the edges or faces of this combination. The straight edge and the two cubes together can now be placed so that one edge of each cube coincides with the straight edge and one end of the straight edge and one corner of a cube also coincide. A distinguishing mark can then be put on the straight edge to coincide with that end of one cube edge which lies between a corner of each cube. The cube nearest the end of the straight edge can then be removed and put on the other side of its fellow cube. By repeating this process a number of marks each with a distinguishing symbol could be made on the straight edge, which would then be called a measuring rule or scale. It is found in practice that any edge of either of the cubes can be put between two marks on the rule so that the cube corners or marks coincide. It is found also that this can be repeated on other occasions and on other measuring rules made in the same way. Suppose now that one of these measuring rules be put to coincide with, say, an edge of a book so that one end of the rule coincides with a corner of the book. The other end of the book-edge can be seen to coincide or be nearest to

one of the marks on the rule. This whole series of actions ending with the reading of the scale mark nearest to the book corner is called direct measurement of the length of an edge of the book.

So far any kind of distinguishing marks, such as letters, may be put against the scale marks or numerals in any order. If, however, the numerals are put in the order of counting, 0, 1, 2, 3 . . . as is usual in most measuring apparatus, the remarkable result is found that the numerical results got from laboratory measurements often agree with numerical results got by the use of metrical mathematics. There is no lack of theories offered to explain these remarkable agreements, and it is quite common to speak of the postulates of such theories as if the postulates were facts. Nevertheless it is well to remember that it is only this *agreement* between laboratory measurements and mathematical calculations that is a fact.

This basic method of measuring a length or distance can be used only with the objects observable by the coincidence method. Such a severe restriction placed upon the use of the word measurement is quite unrepresentative of current practice. Often no distinction in use is made between the words observe, determine, or measure. For example certain elaborate laboratory operations would be called measuring or determining the distance of closest approach of certain atoms in a crystal although atoms have not so far been observed by the coincidence method. Again a piece of research in mathematical physics in which no laboratory operations were used might also be described as a determination of the distance of closest approach of two atoms in a crystal. If instead of using the idea of absolute truth an analysis be made of these two examples in terms of what the scientists can be observed to do, many differences will be seen. For example, the physicist in his laboratory uses an X-ray tube, a spectrometer, and an actual crystal, but the mathematician does not. The fact that each ends with the same re utsl is one of the wonders of science. Unfortunately in such circumstances each does not always end with the same result. It is then that the laboratory

result is taken as basic and the mathematician has to try again.

The treachery of words seems to be responsible for a great deal of the confusion shown in popular science writing where the writers seem to mistake determination or measurement by a mathematician for determination or measurement by a laboratory worker. The effort to sink the differences in a quagmire of philosophy seems so far to have helped neither clarity of thought nor scientific action.

In all research published up to the present, wherever a laboratory measurement is recorded the scientist has always used judgment of coincidence and counting. This statement applies as much to measurements of the molecule, atom, electron, proton, or neutron, on which coincidence observations have never been recorded, as to measurements of bricks. In serious discussion of any scientific measurements the greatest care is needed to distinguish between mathematical and laboratory measurement or determination. Somewhere in the literature it will be found that a mathematical determination is preceded by a long list of If's or Provided that's. The more familiar the subject matter, the more difficult is it to remember that these postulates are postulates and not facts. Every laboratory measurement without exception involves some direct measurement for which coincidence observation is essential, but most of the complex laboratory operations such as those called measuring an electric current need much more than coincidence observation and counting. Operations as different as weighing a deposit of copper, and observing the movement of a magnet or a coil of wire, can both be called measurement of an electric current. If in certain circumstances they all finally give the same number, that agreement is a fact of experience for which we may be truly thankful. The agreement and the idea of an electric current help men to build electric power stations, motors, filament lamps, and radiators. Given a suitable ammeter, calibrated at, say, the National Physical Laboratory, the task called measuring an electric current is very simple, but the preceding work is of extreme complexity.

Direct measurement of a length with a measuring rule is found serviceable in dealing with objects which directly give sense data. Otherwise it is quite inapplicable. Ideas derived from its use are freely used in indirect measurement and the wisdom of the policy can be determined solely by experience. There are no rational grounds for surprise if the actions called making indirect measurements ever give paradoxical results. Measurement seems quite unsuited to the study of pattern properties. Theories can be devised to deal with circumstances in which measurement fails, and in recent physics ideas derived from the wave theory of light have been used for this purpose. The same ideas do not, however, serve to explain the failure to measure things so diverse as pain, hope, Gods, or any of the pattern properties of objects and events.

The patternist offers no theories or other explanations of why the measurements of some objects remain the same from day to day. For all he knows his table and his measuring rule may together vary in length, as judged by other standards at present inaccessible to him.

GOING TO EXTREMES

Where some selected quality under experimental study can be subject to measurement, one device which may be called the principle of going to extremes can be simply described and has often proved useful in the technique of experiment. The hotness of a body can be correlated with its temperature, which can in turn be correlated with direct measurement as, for example, when the coincidence of the top of the mercury column with marks on the glass tube of a thermometer is observed. A selected factor, such as hotness, which can be either varied or kept constant with suitable laboratory devices, is often called a *variable*. If the numerical value of a variable which can be measured is in some experiment made very much larger or smaller, new phenomena are often observed. This forcing of a variable to an extreme value in order to discover new phenomena is used in all the sciences.

In biochemistry the discovery of vitamins and of hormones was due to the observation of certain living individuals in whom the quantity of these substances was abnormally high or low. If the amount of one of the hormones secreted by the pituitary gland of Helen of Troy had become greater than normal, she might well have changed into the bearded lady of the circus. Without the extreme phenomena of the bearded or the fat ladies of the circus the discovery of the necessity of certain hormones to the beauty of Venus or of any other woman would have been much more difficult. The vital necessity of insulin to every human being was not discovered by a study of the millions of individuals who daily produce the necessary quantity, but by a study of those few in whom the quantity produced is extremely small.

Two quite distinct types of result are got by this process of going to extremes. On the one hand an effect detectible only with the aid of delicate apparatus at, say, a low value of the variable, may at a high value be detectible with the naked eye. On the other hand an effect may be quite undetectible below or above a certain value of the variable even with the most elaborate and sensitive apparatus. To get conditions such that the value of the variable is on the correct side of this so-called *critical* value is in such circumstances essential.

These two aspects of the device are readily apparent in physical experiments. If temperature be regarded as the variable and water the subject to be observed, and if a start be made with such a low value of temperature as $-50°$ C., the experimental material might be a block of ice. If this were put in an enclosure at, say, $-45°$ C. small changes of dimensions could be detected with the aid of suitable apparatus, as the ice warmed up. These changes would be much greater if the temperature was raised to $0°$ C. Over this range from -50 to $0°$ C. greater changes of dimensions would accompany greater changes of temperature. If, however, the enclosure were then warmed up to $5°$ C. quite a new kind of result could be observed. The ice would melt instead of warming up to $5°$ C. Increasing

the temperature above this critical value of 0° C. gives a liquid instead of a solid. The liquid differs in many ways from the solid. It will take the shape of the enclosure instead of preserving the original general shape of the solid. The discovery of the phenomena of change of physical state is dependent upon getting suitable temperatures. Without them no amount of delicate apparatus will suffice. Similarly a kettle of cold water placed over a hot flame warms up only until the boiling temperature is reached. Even though the flame temperature be much greater than that of the liquid, the liquid gets no hotter but instead changes to steam. These sudden changes in phenomena when critical values of variables are exceeded are often found in physics and chemistry. An experimenter who finds out how to increase or decrease the value of some measurable quantity a millionfold is almost sure to get some new type of fact when he applies the method in the laboratory or field or observatory.

The idea of studying instances where some variable has an extreme value is easy to express when the variable is measurable. It is not so easy to express where the variable cannot be described as subject to quantitative variations. Nevertheless a very similar idea seems to be generally applicable in scientific research. For example, if human psychology, or a part of it, be regarded as the science of human action, the results applicable to normal human action remained very meagre until men like Freud, Jung, and Adler went to the extremes of studying the actions of lunatics, sex perverts, and those said to be mentally ill. The fruitfulness of this studying of extremes in psychology would be still more apparent if a clear distinction between facts and devices useful for patterning facts were made and if it were realized that the causal type of patterning is not the only scientific type.

When the scientist studies extremes he is as often as not rewarded by both knowledge of abnormal facts and also by a new viewpoint on the normal.

CAUSE AND EFFECT

Just as the word probability has become entangled with the idea of human expectancy, so the words cause and causality are very often connected with the idea of something inside making things happen. In such a use of the terms, a causal explanation is one which tells what internal agent makes a thing happen. It is a kind of 'Why' explanation which cannot be got by any scientific means at present in use. The ideas got in childhood of a cause are always of this type. Later in life, when the child grows up into a physicist, he is apt to speak of gravitation as something which pulls an apple to the ground, something which, by tugging at the oceans, makes the water rise and fall in tides. If he grows up into a psychologist he is apt to speak of an instinct as something in man which makes him do certain actions. In everyday life by far the commonest use of the word cause is to express something which gets inside and makes the observed thing happen.

The early explanations of disease were in terms of demons or spirits which got inside the sufferer and made him ill. To those who use such an idea of cause, together with the Absolute theory of science, the basic ideas of vitamins and hormones as possible causes of disease seem very strange.

But the word cause is used also in another sense to refer to some factor essential to a pattern property of an object or event. If the pattern is in time, then the cause refers to something which precedes an observed effect in a sequence of events. Whether or no a factor is a cause in either of these senses, can be determined solely by the type of experiment now to be described.

Physiologists and biochemists had classified foods in terms of the types of compounds known as protein, fats, carbohydrates, and salts, and were studying the effects upon the body of eliminating one or more of these types from the diet of animals. From the results of these experiments it was possible to prescribe a mixed diet adequate to maintain animals in a healthy condition. At about the beginning of this century Hopkins carried out a remarkable series of

experiments in which two sets of rats were each fed upon the same basal diet consisting of such quantities of the different types of substances as had been shown to be adequate to maintain health. Technical laboratory processes were, however, first applied to purify the substances, that is, to separate, say, the protein from other types of substance. The rats were accordingly fed upon purified casein of milk (protein), lard (fat), sugar (carbohydrate), and salts got from the ash of oats and dog biscuits. In addition one set was given a small quantity of milk daily. The other set had none. The weight of the rats given the milk increased normally. That of the set given no milk decreased after ten to fifteen days. (Fig. 28.)

Before using these experiments to illustrate the idea of cause and effect it may be well to use them as illustrations of other features of experimental technique. The following analysis must be regarded as one way of looking at the work. In no sense is it an attempt to describe what the research workers were thinking whilst devising and carrying out the experiments. It will be noted that they originated in a hypothesis based upon many facts found in previous experiments. This hypothesis might be stated as: 'Young animals will normally grow when they are fed daily with sufficient quantities of protein, fat, carbohydrate, mineral salts, and water.' Devout worshippers at the altar of inference and extrapolation would, I think, have regarded this statement as proved by the previous experimental results. If Hopkins' two sets of rats had grown similarly, many workers would have said that the result was a foregone conclusion. It will be noted that, unlike a postulate in a theory, the hypothesis is stated in terms of things which can be observed by the coincidence method. The sceptical scientist would therefore have wanted the actual observations to be undertaken. Whatever the result, the experiments would not for him have been superfluous.

The experimenter often tries to keep all conditions save one as constant as possible. Hopkins' experiments were done with two sets of rats. Now it is evident that the preparation of specially purified foods is a troublesome and expensive

process. The greater the number of animals the greater is the difficulty of feeding them all with a specially prepared diet. Why not give all the rats the purified diet together with a little untreated fresh milk?

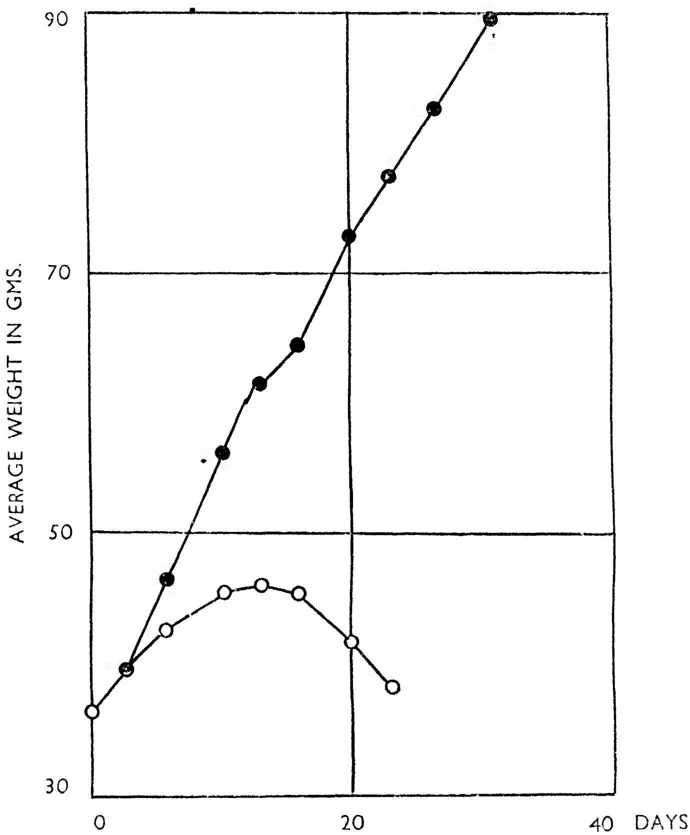

FIG. 28.—Lower curve, six rats on purified diet alone; upper curve, six similar rats receiving in addition 2 c.c. of milk each per day (Reproduced by permission from the *Journal of Physiology*, vol. 44, p. 432. 1912)

Many workers had tried and failed to keep animals for more than a short period on artificial diets. But if the causal type of experiment demands the change of only one factor at a time, the whole environment of experimental

and non-experimental animals must be compared. It is then apparent that experimental animals are kept in a more confined space, they have less fresh air and their diet is more monotonous; factors other than the chemical treatment of the diet are changed. Failing specific results it is unsafe to regard any uncontrolled factor as unimportant or insignificant. In many ways lemons and limes are very similar, but it does not follow that the small differences may not be accompanied by profound differences to health when limes are substituted for lemons as articles of diet. In studying the effects of a monotonous diet it was found that animals could be kept in good health in confinement on a monotonous diet of untreated milk or egg.

At this point it is well to remember that if two objects or events are observed by different people the different individuals do not notice only the same differences in the two objects or events compared. Each individual may see some difference not noticed by the other observers. If an observer makes any change whatever in repeating an experiment how then is he to know that he has altered only one factor (apart from the changes of time in a sequence of experiments)? So far as I am aware, there is no way of knowing all the factors which have been changed. But in practice it is found that experienced workers notice sooner or later any change which is observable by the coincidence method. To any claim made by an experimenter that he has changed or kept unchanged anything unobservable by the coincidence method, the patternist and the sceptical scientist alike add a grain of salt and treat the claim as they do any other sacrifice made on the altar of inference and extrapolation.

Since no man can be sure that in altering one condition in an experiment he has not unconsciously altered others, all causal experiments are done in duplicate. In the Hopkins experiment all the rats were fed with the same basic diet and were treated in all other respects as nearly alike as possible under the given laboratory conditions. The experiment of giving no milk to some of the rats would be called the *control* or 'blank' or 'blind' experiment. The

addition of milk to the diet of the remaining rats, or the absence of the milk from the diet in the control experiment, would be called the cause of such observed results as the different rates of change of weight of the rats. When the terms 'cause' and 'effect' are used, the observed changes would be called the effect produced by the cause. From this typical example it is clear that the word *cause* is used to describe a set of conditions which is accompanied or followed by some effect (observable by the coincidence method). When a cause has been determined by this kind of experiment the knowledge helps us in action, for it is found in practice that the prescribed set of conditions gives the same effect each time they occur after the original observations.

CAUSE AS INTERNAL AGENT

In everyday life the word cause is often used to mean an agent. The idea of an agent getting inside and making things happen seems to be an animistic idea related to everyday experience. 'Lying on a writing-table is a pen. A man picks it up and starts to write. The pen moves across the paper and a sequence of changes can be observed. If the man had not grasped the pen none of the changes would have been observed.' This kind of reasoning seems to be contrived to explain any kind of observed effects. 'If any change is observed, a man or something like a man, an imp, a demon, a fairy, or a god, must have made it happen.' This way of explaining effects in terms of an agent is logically sound. By some it is regarded as the only satisfactory kind of explanation. Complex effects can be explained by its use in a piecemeal or little-by-little manner. When insistent demand is made for an agent-explanation anything will apparently serve as agent. 'If an apple falls to the ground something must have either pushed or pulled. If an individual carries out any observable action something must have made him do it. There can be no effect without a cause.' The biographer examines letters written by and statements made about Charlotte Brontë. Some of her

actions are then explained by finding inside her an 'inferiority complex.' "That colossal inferiority complex which she called 'Reason' took her to task brutally and jeered at her whenever the faintest hope glimmered in her heart."*

Whenever any of these agent-causes are examined in the light of agreement between observers, they prove to be a kind of circular explanation. On finding that an inferiority complex cannot be observed by the coincidence method, the patternist asks what tests, based upon agreement between observers, must be applied to find out whether or no a person 'has' an inferiority complex. He is given a list of actions which will be observed with abnormally high frequency if the person has an inferiority complex. Having found a person possessed of this inferiority complex, the patternist next asks for a list of the actions to be explained as caused by the inferiority complex. He is given precisely the same list. The agent-cause and the effect are indistinguishable by any method based upon agreement between observers.

This result is equally applicable to causal explanation of physical phenomena. The observed effects caused by a gravitational field are indistinguishable from those tests, based upon agreement between observers, which must be applied to find out that a gravitational field is acting. In these comments there is no suggestion that the inferiority complex or the gravitational field are other than extremely valuable patterning devices for dealing with certain facts. But the surreptitious transformation into an agent of a cause discovered by the causal type of experimenting is a piece of intellectual dishonesty.

No scientific device whatever has yet given an explanation of why anything happens, in the sense of what agent makes it happen. The successful causal type of experimenting tells how things happen, not why. To some this statement may seem like a confession of failure. It is, however, no confession of failure if action is taken as basic. Causal experiments tell what must be done to produce certain effects.

In policies of action the experimental results help as much

* I. C. Willis, *The Brontës*, p. 93. London, 1933.

by telling what may be left out as by telling what must be done. It may be, that, according to the legend of the discovery of quinine, a fever-stricken man who later drank the waters of a certain pool in a tropical forest was cured of his fever, whilst his comrade, similarly stricken, who failed to drink the waters, soon died. Until a highly complex series of causal experiments has been completed and has shown that a similar cure may be effected by taking quinine, it is unsafe to depart in any detail from a repetition of the whole circumstances of the actions preceding the recovery of the sick man. For all we know it may be necessary to drink the waters at the same intervals of time, in the same amounts, lying in the same position, and to pray to the same Gods. To effect a cure it may even be necessary that the stellar configurations shall be the same. Although the manner in which quinine acts upon the malaria parasite is still unknown, the results of the causal experiments help in determining what to do in order to cure malaria. Similarly the causal experiments upon cancer will fail to tell why cancer cells are produced, but, when successful, they will tell under what conditions the normal growth of cells in the body changes to the abnormal growth characteristic of cancer.

In practice the causes discovered by experiment are not unique conditions necessary to produce a given effect. Rickets may be avoided either by giving a child an adequate amount of vitamin D in the food, or by exposing its body to ultra-violet light or sunlight. No guarantee can be given by the scientist that in the future some different set of conditions will not be discovered to give the same or similar effects. The same effect is always got from the same conditions, but may also be got by other conditions. So far as can be seen at present the task of scientific research is therefore endless.

In considering the causal type of experiment and comparing its products with the causes referred to in everyday life, two striking differences are apparent. In everyday life a cause is regarded as an agent and 'control' experiments are not used. In the scientific laboratory a cause is a set

of conditions which precedes or accompanies the effect, and 'control' experiments are essential. Few research workers cannot but have been sorely tempted at some time to shirk part of the work involved in control experiments. The more elaborate, difficult, and expensive the original experiment, the more so the control experiment. The necessity for control experiments may double the work to be done. Many a research worker is deterred from doing an experiment of great interest to him, because he cannot devise a suitable control experiment. He may know that in all the control experiments which occur to him as practicable the circumstances will differ in more than one factor which might affect the particular phenomenon studied. Ideal control experiments are not in fact always devised. In such circumstances it is specially desirable that the research worker report what he has done in terms of facts, instead of expressing in terms of inference and extrapolation what he may think he has done. For the sceptical scientist the necessity for control experiments, at times, converts research work into a form of torturing slavery.

Excellent everyday examples of the neglect of control experiments in making claims to causal results are seen in politics, 'cures' of illness, and all subjects involving superstition or belief or faith. At a general election many events happening during the life of the previous government and favoured by the majority of electors are claimed to have been caused by the actions of the party in power before the election. A patent medicine taken before recovery is claimed to have caused the recovery. The failure of an enterprise started on Friday is claimed to be caused by the unlucky nature of Fridays. Rain following a prayer for rain, is claimed to be caused by the prayer. It would fit the facts better to regard these claims as statements made so that many men will carry out such actions as putting a cross in a certain position on a piece of paper and so on. The politicians and vendors of patent medicines would not, I think, in their non-professional moments, claim that they were establishing knowledge.

Enough has been written about the causal type of experi-

menting to show that in the present stage of knowledge it is always difficult, and not infrequently quite impracticable, in biological problems. As many of the problems of civilization are biological problems, it is not surprising that this one device of scientific research, the use of 'control' experiments, is in general quite inapplicable. Failing 'control' experiments, no cause can be proved (i.e. tested).

At a time when none but the Absolute theory of science had been devised, the term *crucial experiment* was invented. A crucial experiment is one which gives facts inconsistent with one hypothesis and consistent with another. The Hopkins experiment might be called a crucial experiment in that its results were inconsistent with the hypothesis given on p. 292, and consistent with Hopkins' hypothesis about accessory food factors (vitamins). The experiment is crucial in that it enables a choice to be made between two alternative hypotheses. On what grounds is the choice made in interpreting the result of any crucial experiment? The choice is always made so that the hypothesis which does not fit the facts is rejected. The results of crucial experiments tell nothing whatever about the future. At the time the experiment is made, the rejected hypothesis is an unsuitable patterning device for use with the available facts. In the future, when more facts are available, the rejected hypothesis may be found useful again. In the past both the particle and the wave theories of light have been temporarily rejected because of the results of crucial experiments. The facts discovered in all carefully performed and recorded crucial experiments remain unchanged with the passage of time; the hypotheses may or may not remain in active use. In the quest for the Absolute, the crucial experiment seems to have proved, not a divining rod, but a broken reed.

SOME SPECIAL DIFFICULTIES OF BIOLOGICAL RESEARCH

Variation of Biological Material.—If the previous discussion has shown the necessity for control experiments it has not shown the necessity for using two sets of rats in the Hopkins

experiment. Why not have chosen two healthy rats, as similar as possible, and let one of them serve in the control experiment? Unfortunately for the simplicity of biological research, wide variations are always found in the behaviour of individual biological units when they are placed in environments which, to the experimenter, seem the same.

Human reaction to drugs provides many examples of this. A dose as small as 1 gr. of aspirin has been known to produce asthma, associated with extensive wheals on

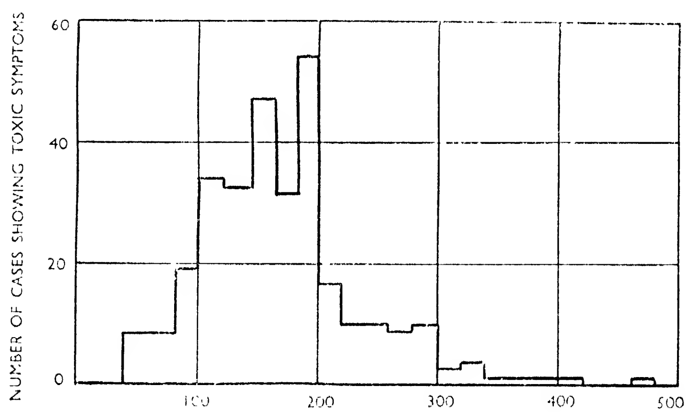

FIG. 29.—Diagram showing 'scatter' of the results of measurements of the amount of sodium salicylate which could be taken before toxic symptoms appeared. 300 males. (Hauzlik, 1913)

the skin. In measuring the amount of sodium salicylate (a drug to which aspirin is related) which could be taken before toxic symptoms such as dizziness or nausea appeared, wide variations were found. Out of 300 male patients, 200 responded to doses of between 100 and 200 gr. Some, however, responded to doses as small as 50 gr., whilst others could tolerate a dose as large as 500 gr. This 'scatter' of the results is shown in Fig. 29, taken from the work of Hauzlik* in 1913. It might be supposed that the variations were due to the variation in age, weight, and disease of the individuals. In Fig. 30 is shown the kind of

* Hauzlik, *Journal of American Medical Association*, 60, p. 957.

result obtained by Paxson* in 1932 with 55 healthy women of child-bearing age when the doses were calculated in mgm. per kilo body weight. The 'scatter' of the results is but little reduced. The same variation in less dynamic aspects of individuals was shown in Chapter IV, and the general variation must be accepted as a fact.

A striking example of a biologist's caution in experimental research, where the individual variations in experimental animals may be so great, is shown in the planning of one

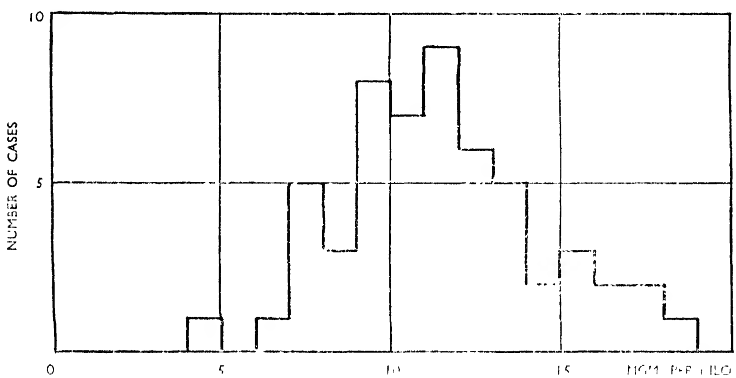

FIG. 30.—Diagram showing the 'scatter' of the results of dosage of sodium amytol (mgm. per kilo.) needed to produce adequate narcosis when given by slow intravenous injection to 55 obstetric cases. (Paxson, 1932)

of Hopkins' later experiments. The experiment already described was repeated with two sets of eight rats each. Then, eighteen days after the weight of those rats without milk had begun to decrease, fresh milk was added to their diet and was discontinued from the diet of the other set of rats. The weight of the first group soon began to decrease, whilst that of the second set now began to increase (Fig. 31). The same effect, decrease in weight on a purified diet from which fresh milk was absent, was produced with *either* set of rats. Individual idiosyncrasies of the first set of rats did not account for their loss of weight.

It might be thought that this highly ingenious experiment

* Paxson, *Anesth. and Analg.*, 11, p. 116.

could be used to formulate an absolute rule, that in biological research the experimental conditions applied to the two sets of animals must be reversed half-way through the experiment, so that the phenomena studied occur sooner or later in both sets of animals. But the behaviour of a biological unit varies in some respects with its previous history. From the very nature of the technique this previous history cannot

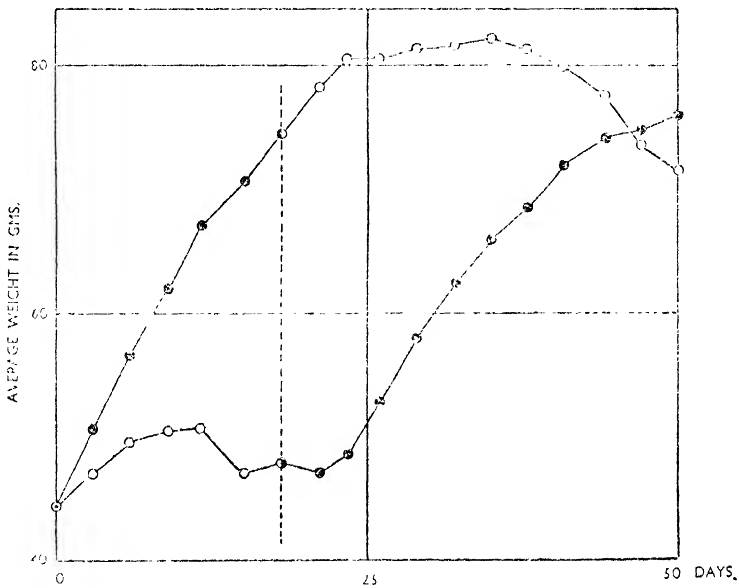

FIG. 31.—Variation of average weight of a rat (in grms.) with time (in days) when diet changed. Each curve relates to experiments on eight similar rats whose diet was interchanged on the eighteenth day (marked by the broken line). (Reproduced by permission from the *Journal of Physiology*, vol. 44, p. 433. 1912)

be the same for both sets of animals. Then again some of the animals may die in the course of the first part of the experiment. The pursuit of absolute rules in experimental research has so far been as unsuccessful as any other pursuit of the absolute.

Organization and Causality.—Many of the properties of living things are pattern properties, depending upon the relation of parts to a whole. The idea of causality which has served

well in many scientific studies, is an unsuitable patterning device, even when extended to the idea of multiple causation, for dealing with pattern properties. A new technique is being developed, but the dogmatic teaching of older ideas is apt to hinder progress in this direction.

Time Factor.—Many biological processes are slow, and the time taken to do some experiments depends upon the normal period of life or of cyclical changes, for the type of animal used. Increasing the number of research workers, or improving their facilities, would not reduce the time taken to do some biological experiments. All these technical difficulties are not, however, the only obstacles to be surmounted by the biologist.

Emotional and Ethical Opposition.—Many biological problems of interest to research workers and of tremendous practical utility, relate to dynamical problems of living things. They relate to changes characteristic only of living things as such. So far no man has discovered how to study these phenomena of life by studying only inorganic material and dead organic material. Some of the experiments require that the experimental animals shall remain alive during the experiment. On this account some men apply assessment of value and follow this by vigorous use of the Should-Ought mechanism to give the results that such experiments 'should' not be carried out. Having reached this result they then spend considerable amounts of money, time, and effort, in trying to prevent or impede the research workers doing the experiments.

The scientist as such cannot find out whether the experiments should be done, but so far he has been able to establish certain kinds of facts only by the aid of the experiments. Without these facts many human beings now alive would be dead. Again the scientist as such cannot find out if these men would be 'better' dead than alive. It might be thought that the delicacy of chemical and physical tests was now so great that it would be quite unnecessary to use living animals for other than pioneer research. Unfortunately certain medicinal drugs have to be tested or standardized in animal experiments because the chemical and

physical tests so far devised fail to distinguish samples which are toxic when given to human patients from those which are not toxic. Quite apart from any ethical considerations, efforts are continually made to eliminate the necessity for this kind of animal experiment on account of the greater expense, time, and technical skill involved.

As the present discussion is confined to difficulties of experimental research in biology, I am concerned, not with values, but only with the practical influences of the actions claimed by certain people to be based upon assessment of value. As the experiments involved are, in non-technical literature, often called *vivisection* experiments, it is well to understand in what senses the term is used. To most people it suggests some such major surgical operation as removal of the appendix. But this is not the only sense in which it is used. The law governing the experiments does not use the term vivisection but relates to certain experiments which for medical, physiological, or other scientific purposes are done upon live animals.* All experiments covered by this law are, by some opponents of the research, called vivisections. Hence pricking the skin of an animal by insertion of a hypodermic needle may be called an act of vivisection. This means that, if the term is applied to the human animal, then the man suffering from diabetes, who is enabled solely by the use of insulin to keep alive and able to carry on his work, does so by vivisecting himself twice a day in order to inject the insulin. To pierce a woman's ears so that she may wear ear-rings, no special legal precautions are necessary. To pierce the ear of a live rabbit, the experimenter must obtain a licence legalizing the action in one particular institution only, and providing that the rabbit be kept under the influence of an anaesthetic of sufficient power to prevent its feeling pain. If the rabbit were treated as would the human animal, no anaesthetic would be used and then an additional certificate would be necessary.

Those who have lost a friend or loved-one through death from an incurable disease, can scarcely avoid some impatience with biological research workers for failing to

* Full details are given in *The Law Reports*, vol. xi, pp. 459–64. 1876.

understand these diseases sufficiently well. In fairness to these workers, several facts must be remembered. The number of workers on such a disease as cancer throughout the whole world is exceedingly small. In general vastly greater amounts of time, money, and effort are spent upon research designed to destroy human life than upon research designed to cure disease or promote health. Then again, some of the time, money, and effort which might otherwise be spent upon biological research has to be devoted to ensuring that in carrying out the experiments, the workers are not breaking the law of the land in which they may be working. Sir Arthur Keith writes: "We permit botanists to carry out any experiments on plants they care to devise because we assure ourselves that plants do not feel; they only live. It is when we propose to experiment on sentient animal matter that an additional factor enters into our inquiry. . . . The physicist has to satisfy only a single or scientific ideal; . . . the experimental biologist . . . has to satisfy an ethical as well as a scientific standard. To serve these two ideals, to harmonize them, entails a conflict of mind of everyone who seeks to extend biology by experimental research "*

* Sixth Stephen Paget Memorial Lecture, p. 2. 1932.

CHAPTER XVI

THE FUTURE OF EXPERIMENTAL RESEARCH

AMONGST man's major activities scientific research, as it is now practised, is quite a new-fangled device. In science, the widespread use of experiment has come about within living memory. In the early 'seventies there were only eleven physics laboratories in the British Isles. Now there are more than three hundred. It would be surprising if such rapid growth were unaccompanied by its own special problems.

As I see it, some of these problems will, in the future, affect the whole of civilization. Yet at present they can be readily apparent only to those in intimate contact with the practice of research. The selection of topics for discussion in this chapter has been made therefore so as to direct attention to subtle but powerful factors influencing research and to exclude problems already widely recognized and treated elsewhere.*

An intimate contact with facts is unavoidable in experimental work. Some men find the contact helps clarity of thought. Helmholtz, in a letter dated March 28, 1869, wrote: "I found that so much philosophizing eventually led to a certain demoralization, and made one's thoughts lax and vague; I must discipline myself awhile by experiment." Other men find the contact unessential if not even disconcerting. When Maxwell had arranged an experiment to show an optical phenomenon (conical refraction), he asked Todhunter if he too would like to see the phenomenon. Todhunter replied: "No, I have been teaching it all my life and I do not want to have all my ideas upset be seeing it." One of Helmholtz's colleagues urged that "a physiologist had nothing to do with experiments, though they might be well enough for the physicists"† and that the lower experimental side should be left to others. Even nowadays there

* See the *Reports* of the Department of Scientific and Industrial Research (1916 onwards) and of the University Grants Committee.

† The Königsberger *Life*, p. 66.

are some who regard the performance of some experiments as not 'quaite naice.'

IS RESEARCH GETTING MORE DIFFICULT?

Research workers can sometimes be heard to say that 'all the easy researches have been done,' that the day of the individual worker is over, and that specialization is essential. It may therefore be asked whether the kind of problems now studied make greater demands upon the intellectual or manipulative skill of the scientist. The simplest way to deal with this question would be to present a study of the work of many living scientists. Since, however, scientists are not pure-reason machines, such a study could be presented only by a man with an adequate private income and no wants other than those purchasable by use of his income. There is, however, no indelicacy in referring to one living scientist, Einstein. He is known so widely solely for his work on Relativity that his fame in this subject might at first sight be regarded as 'proving' that nowadays a scientist must specialize. But I have on several occasions heard his work discussed informally by competent scientists, who all agreed that if his work on Relativity were to be eliminated, the rest of his researches would still have given him title to world-wide fame amongst scientists. Using Einstein, then, as a kind of 'control' experiment, it is evident that, given the man and the environment, restriction to one line of research is still unessential.

The question of the manipulative skill needed nowadays can be dealt with adequately without naming any living scientist. One of the earliest microscopists, Leeuwenhock (1632–1723), worked with a single lens which he prepared himself. Yet his skill was so great that science students find it by no means easy to discover with the modern instrument all the details which he recorded. His observation of bacteria has even been doubted by some experienced workers, although he gave drawings of the three characteristic types. To repeat Bunsen's research on the cacodyl compounds (1837–43) with Bunsen's facilities and means of analysis and

to escape death by poisoning would tax severely the skill of many present-day chemists. Equally would the physicist's skill be taxed in repeating, with the original resources, such experiments as Galvani's (1737–98) on the contact of two dissimilar metals or Coulomb's with the torsion balance (1777). It would be extremely difficult so to define the term 'intellectual and manipulative skill' as to make the facts fit the claim that greater intellectual or manipulative skill is nowadays needed in research than formerly.

THE PROBLEM OF SPECIALIZATION

How then does it come about that there is so much said of the necessity for specialization in research? As I see it the fundamental difficulty forcing specialization into much present-day experimental research lies not in the nature of the problems and not in intellectual disabilities, but in the inaccessibility of expensive but essential facilities.

To make this clear many factors affecting experimental research must be considered. The general reader may be inclined to suppose that specialization in science is a problem affecting scientists, but having little influence upon his own life. I submit that it is a problem affecting the whole of civilization, for it is generally recognized that present-day civilization depends upon the practical application of the results of scientific research. Have those applications leading to the greatest changes originated in specialized research? Undoubtedly they have not so originated. To take one example from one problem only, the growth of new industries; the electrical industry, with its profound influence upon civilization, arose not because electrical lighting, power, heating, methods of transport and communication, were imagined and then realized by specialized research, but because Michael Faraday and other research workers had the facilities and the freedom to 'go their own way' in research.

What I call the problem of specialization rests then upon these two facts. Firstly, specialization is increasing in all scientific research and secondly, specialization is unfavour-

able to that kind of research which, when applied to practical affairs results in the greatest changes. It may, therefore, be asked what are the chief factors affecting specialization in experimental research and what are the least disturbing changes which might be made so as to favour non-specialized or free-lance research?' *Exceptions can be found to each of the statements*, and the discussion must be regarded as expressing the results solely of my own analysis of present-day tendencies.

RESEARCH CENTRES

Throughout the world research is carried out in three types of institution:

(i) *Universities.* In this country it is widely recognized that "research no less than teaching, is a primary function of these institutions"* and the terms of appointment of a member of the staff usually include some reference to research as one of the duties of the appointment. The work is financed partly by endowment and partly by grants from state and local government authorities, who place no restrictions upon the topics of research.

(ii) *Research Institutions.* These are dissociated from teaching and are devoted wholly to research, or to research testing, and maintaining accurate standards. They include the National and the Lister Institutes for Medical Research in London, the Rockefeller Institute for Medical Research in New York, the Pasteur Institute in Paris, the Davy Faraday Laboratory and the National Physical Laboratory in London, the Physikalisch-Technische Reichsanstalt in Berlin, the laboratories of the Research Associations† in this country, and the twenty Kaiser Wilhelm Institutes in

* Notes on the Grants to Research Workers and Students. Department of Scientific and Industrial Research, p. 4, 1923, and later issues. See also S. G. Robertson, *The British Universities*, 1930, and Appendix xxiv, "Centres of Scientific Research and Information," of the annual *Universities Yearbook* (pp. 869–933. 1935).

† For full details see the Annual Reports of the Department of Scientific and Industrial Research and of the Medical Research Council. Published by H.M. Stationery Office.

Germany. The majority of these are now financed from national funds, and from contributions made by the industries concerned. Some of the medical laboratories are maintained by endowments.

(iii) *Industrial Laboratories.* These are devoted to research upon problems of the industry concerned. They are financed entirely by the industries which also control the topics studied and the publication of results. The work is usually highly specialized but the facilities are often excellent.

It is evident that the chief centres of non-specialized research are the universities and a very few research institutions (chiefly medical).

THE COST OF RESEARCH

Nowadays research is much more expensive than formerly. Some idea of the sums involved to equip a laboratory for work of one restricted type may be gathered from an example taken from physics. In 1935 an ordinary X-ray set cost about £200, an X-ray spectrometer about £150, a recording microphotometer about £300, and all three may be needed in one piece of research done by an individual. In actual practice larger sums would be needed for 'extras' and it is doubtful if less than £1,000 would be involved to get the research efficiently started. I estimate the average cost of a 'paper' in academic physics at between £200 and £1,000. The result was got from consideration of research in universities so that only *one-half* of the salaries and departmental laboratory equipment maintenance grant was included. Allowance was, however, made for the cost of maintaining the laboratory buildings and for installation of all equipment. From institutions devoted entirely to research more papers would be published, but the *whole* salary and maintenance cost would have to be included. I was told that the result of an estimate made two or three years ago, of the average cost of a research paper published from a famous biological laboratory, was very similar. It may therefore be taken that the minimum average cost nowadays of a research paper covering one year's work is of the order £500.

So far, then, it is evident that universities are favourably situated for non-specialized research because in any science the topics to be investigated are not restricted by those controlling the finance of the research. If, however, the financial resources of universities are severely limited, then the advantage of freedom from restriction is lost in the absence of expensive but essential facilities. The tendency is to approach the state in which the university workers have freedom without facilities and the industrial workers have facilities without freedom for free-lance research.

Referring to industrial research, Freeth states: "The imposition of the cost of research upon the cost of manufacture is nearly always one which is to a certain extent resented by technical men of affairs."* "The average manufacturer is impressed with the importance of quick returns; he cannot afford to wait. The managing director of one manufacturing firm recently told us that he had no interest in research which did not produce results within a year. If science can help him to overcome the difficulties that cross his path from day to day, he welcomes her. He wants a handy servant, not a partner with ideas of her own."† The prime function of an industry is neither to make knowledge nor the products of the industry. Its success is measured in terms of money and its research laboratory is justified only in so far as it helps to maintain or increase profits.

On the other hand, the two prime functions of a university are to make and teach knowledge. At this point it seems reasonable therefore to neglect industrial laboratories as not being primarily centres of non-specialized research and to concentrate attention upon universities.

Monetary Influence Upon University Research. The income of each university is derived from endowments, grants, and fees. Oxford and Cambridge are much richer in endowments than are the newer universities of England, whose "income

* F. A. Freeth, "Industrial Research," *Journal of the Society of Chemical Industry*, vol. 48, p. 1086. 1929.

† Report of Department of Scientific and Industrial Research, p. 10. 1916.

can normally only just cover their expenditure."* The student fees of provincial universities could not be increased without severely handicapping many of the students. For the present it will suffice to note that in this country an excellent scheme for controlling university finance is in operation, but that the money available for research is, in general, too small to favour free-lance research.

In each of the following examples it will be noted that the main factor favouring specialized research is the expense of experimental research in relation to the available university funds. It was suggested that the cost of apparatus needed for a certain type of X-ray research was about £1,000. But it may be possible for several workers to use the same apparatus, since the apparatus is not needed by a research worker whilst he is interpreting his results. There is a tendency then so to plan the research in a university science department that, if a piece of expensive apparatus is necessary, several workers shall work upon the same lines, and no other expensive apparatus will be required. This necessarily favours specialization.

Normally the original choice of the expensive apparatus is made by the professor, who is therefore able to do free-lance research. It does not follow that one pair of hands is adequate to carry out, in a reasonable time, all the free-lance research devised by the brain belonging to that pair of hands. A dozen pairs of hands may be necessary. "There is a remarkable consensus of opinion that any man, however great his genius, is probably not capable of directing the work of more than fifteen people . . . to obtain the maximum results."† I know of no university professor who has ever had a dozen paid research assistants. Owing to the lack of funds the average number of paid assistants is in this country nearer one to each professor. From the very nature of the situation the professor is unavoidably faced with a dilemma. Having chosen a research problem to the best of his ability

* H. T. Tizard, "Science at the Universities: Some Problems of the Present and Future." British Association Report, pp. 207–222. 1934.

† F. A. Freeth, "The Influence of Technique on Research," *Journ. Inst. of Metals*, p. 378, vol. xliii. 1930.

he cannot guarantee that its study will be inexpensive. If he needs extra pairs of hands, from where is he to get them? In practice the only sources are usually his staff and his research students. This may seriously detract from the possibility of any scientists other than professors doing free-lance research.

In order to save money, the practice of universities is now, with increasingly fewer exceptions, to offer no lectureships for open competition, so that entry to a university staff is by means of an assistant lectureship. What factors affecting specialization in research influence the choice of candidates for admission to university staffs as assistant lecturers? Normally all candidates have had a little experience (and no training) in the teaching of their subject. In that branch of their work they may be taken as equal. In the research which forms the other part of their duties, their experiences will differ. A university science department has a very limited sum of money to spend annually upon its teaching and research laboratories. In choosing among applicants the professor has to remember that his own duties include the promotion of research to the best of his abilities. He may well find that therefore he cannot conscientiously appoint an applicant whose research would need additional expensive apparatus or would prevent him from helping in the research already in progress in the department. This is a dilemma arising solely, as I see it, from lack of money. If the funds are barely sufficient to pay for the research already established in the department, then to appoint an assistant-lecturer needing other expensive apparatus would necessitate robbing Peter to pay Paul.

But is it only trained research workers who can help most in a piece of research? Undoubtedly many a research worker would be helped far more by skilled technicians, mechanics, instrument-makers, and glass-blowers than by other research workers. The inadequacy of non-academic assistance in universities is widely recognized. As a result many a university research worker has to carry out all kinds of tasks in which he is in general quite unskilled. Furthermore, the market price of skilled labour for the same tasks is usually

very much lower than the research worker's salary. A curious feature of this is that the control of university finance in the provincial universities is partly in the hands of local industrialists. It is commonly considered unsound business practice to pay up to ten times the market price for labour, and yet in universities it happens not infrequently that technical labour requisite for a research is carried out by the research worker himself who is thus, for that part of his research, being grossly overpaid and is robbing the university of valuable research time. Freeth suggests that the inadequacy of the technical assistance is due to "lack of realization on the part of the people with the power to remedy it, and a certain apathy on the part of a few of those who would benefit by it." "A scientist who has not access to a good workshop (i.e. trained mechanics and their facilities) does not know what he has missed, his experimental and even his mental work are handicapped by a constant succession of inhibitions due to his lack of mechanical aids." The net result is to impede free-lance research and to favour highly specialized research.

EMOTIONAL FACTORS

As it would be misleading to suggest that monetary factors alone are forcing scientists into extreme specialization, the main discussion will be interrupted here to consider certain emotional factors one of which, however, relates to expensive research.

Types of Research. It will later be of service to refer to a rough classification of research work in terms of two extreme types. The main characteristics of the first, called 'essentially new,' pioneer (or *bahnbrechend*) research, have been explained in Chapter I.

Another extreme type of research is known amongst scientists by the colloquial terms 'pump-handle,' 'safety-first,' or 'pot-boiling' research. These terms can be explained by reference to the Hopkins experiments in which the addition of a small quantity of milk to a basic diet make the difference between health and illness or death. No sooner is

this remarkable difference following a small change in diet made apparent, than the question arises: Could any other substance be substituted for milk, to be followed by the same results? To answer this question Hopkins' experiments must be repeated hundreds or thousands of times, using in turn each of the substances to be tested as substitutes for milk. These repetitions of Hopkins' experiments would be called in colloquial laboratory language, 'pump-handle' or 'safety-first' researches. Given the requisite technical skill, the greater the industry in this kind of research the more the established results (facts) just as, under normal conditions, water flows so long as the pump handle is worked. There are no risks of getting results difficult to interpret, or difficult to present in a scientific journal, for which reason a scientist who publishes much of this kind of research is called a 'paper-merchant' or a 'paper-machine.'

The terms are, clearly, emotionally coloured. Those who would like to regard research workers as 'logic engines' would have the greatest difficulty in explaining how it comes about that the same research worker who on one occasion pokes fun at a man's doing one kind of research, upon another occasion makes use of its results as essential to his own work. To avoid misunderstanding, I wish to emphasize that 'pump-handle' research is not necessarily easy. The same manipulative skill is needed as in the pioneer research on which it is based. Furthermore, 'pump-handle' research is essential, for a world of science in which pioneer research was unaccompanied by a vast amount of 'pump-handle' research would quickly become the extrapolator's* paradise. No sooner is a new type of physical constant discovered, than some ninety elements or a quarter of a million chemical compounds lie waiting to be measured in the new way.

It is possible to tell beforehand if a proposed experiment

* I use the term *extrapolation* to mean guessing a fact, that is to say, taking for a fact that which has never been so observed, although from its supposed nature it could be observed by the coincidence method. Extrapolation is a special form of inference. In other kinds of inference the supposed nature of the thing inferred is such that it cannot be observed by the coincidence method.

is of a new type, but it is impossible to tell beforehand that the results will be essentially new. Although all pioneer research uses some pump-handle results none has, so far as I am aware, originated in a pump-handle series of experiments. Pump-handle research is always highly specialized and confined to regions already delimited.

One feature of highly specialized research is that the language, in which it is published, becomes so highly specialized that a general worker in the particular science cannot understand a paper in the subject without seeking elsewhere for an explanation of the notation. Highly specialized papers therefore tend to be 'exclusive.' When assessment of value is applied, 'superiority' is more easily attained the more delimited the field. Whether in a scientific discussion this is regarded as favouring highly specialized research depends upon the particular theory or scientific explanation of human action used.

Emotional Objections to Expensive Research. There is one remarkable objection sometimes made to expensive research. One scientist put it: "Any fool can do research with £1,000 worth of apparatus. It takes a clever man to work with simple apparatus." The basic idea or emotion accompanying this attitude seems to be a desire to 'show off' skill. But this skill is only displayed if the *same* work is done with simple and therefore inexpensive research as can be done by the more expensive methods. The expense of a piece of research has no scientific connection with the results. A fact is a fact whether it be obtained at a cost of £1,000 or of £1. Research workers are so made that they admire most results got by the simplest means. Experienced research workers dislike complex apparatus because there is in it so much to go wrong. For them, complex and expensive apparatus is tolerated as a necessary evil. But to limit the physicist to the use of Grove's cells, temperamental quadrant electrometers, and Sprengel pumps is to set limits to the bounds of physics.

The second type of emotional objection to expensive research relates particularly to financial control and illustrates the great danger to free-lance research which might

follow democratic control by scientists of money available for research. The facts relate to a series of recent events in the scientific world which were reported in *Nature** and the chief newspapers in 1935.

It happened some years ago that a research worker wished to do a piece of free-lance research in physics of the type called in the previous chapter 'going to extremes.' To push his experimental work into an extreme region of the field in which he was interested he had to work on almost an engineering scale. The large sums of money necessary were provided by a purely scientific body. After some years had been taken to develop the necessary technique, attention was directed to the research by political complications which prevented the worker from continuing the research. At this stage the facts were that large sums of money and much time had been spent upon the research, a considerable advance in technique had been made, but the results obtained with this new technique were not 'essentially new' in the sense explained in Chapter I.

No man who came into contact with research workers at the time could have failed to notice two aspects of the comments made by the majority upon the affair. Freely applying assessment of value, they asserted that firstly the large sums of money spent were not 'justified' by the published work. No account was taken of the fact that the new technique had as yet been applied to only a small field of work. Secondly, their low assessment of the results obtained with this limited application of the technique was stated to be reached because the results were not 'essentially new.'

From the scientific standpoint, comment upon the first aspect is irrelevant, since it relates to assessment of value. On the second aspect, critics contended that the expenditure was not 'justified' because the results of the experiments agreed with those expected. Furthermore, the results 'expected' were those based upon the simplest extrapolation. No critic suggested that the experimental results could be got by any simpler or less expensive experimental method.

* Vol. 135, p. 755. *The Times,* April 29 and May 1, 1935.

In so far as could be observed through the fog of emotion and inference surrounding the commentators, it appeared that extrapolation was considered to be an adequate scientific substitute for expensive or difficult experimental research.* Furthermore, the majority of scientists seemed to dislike intensely the idea of any one research worker more than another having access to very expensive facilities, no account being taken of the marked differences in expense of research in different subjects.

FACILITIES AND FREEDOM NEEDED FOR FREE-LANCE RESEARCH

The whole discussion will now be reviewed. 'Essentially new' results, when applied to practical affairs, are followed by the greatest changes in civilization. Free-lance research is favourable to the discovery of essentially new results, and specialized or restrictive research is unfavourable. The intellectual or manipulative skill needed in research is nowadays no greater than formerly, but the expense of present-day laboratory facilities is much greater. Although certain emotional factors favour specialized (including 'pump-handle') research at the expense of free-lance research, the main promoters of specialization are lack of funds to buy essential facilities or lack of freedom to use the facilities. Universities are in general best fitted for free-lance research and for non-specialized research needing the co-operation of two or more sciences. University research is, however, hampered by lack of funds, especially in the more advanced sciences, where technicians could often help more than additional academic staff.

RESEARCH AND CHARITY

Two kinds of research are quite independent of charity: industrial research carried out in the private laboratories of

* It must be distinctly understood that these comments of mine refer only to observations made upon scientists with whom I happened to come into contact. The finance of this particular research was fortunately under the control of leading scientists of the country concerned

firms, and research directed to devising or improving means of destroying human life or property in circumstances where the destruction is not desired or is resisted by the owners. All other kinds of research, both in universities and in specialized research institutions, depend primarily for their foundation, or their continuance, or both, upon charity. It is difficult to see how universities could have been established and maintained without the generosity of those few individuals with vision and ideals enabling them to pay the piper without calling the tune. When extra assistance had to be sought from the state it was feared that such aid might partially restrict university ideals. As it happened, British genius for compromise won and the universities receive state aid but are able to serve the community, as Tizard points out, "untrammelled by political influence; trusted guardians of public money." Not so long ago British scientists looked with envy at the facilities for research provided by the state in Germany. Now it appears that those facilities may be used only if the scientist will restrain his passion for coincidence observations and patterns to those acceptable to a few individuals.

Nowhere in the world is a scheme better adapted to promote free-lance research than that successfully used in the British universities. The University Grants Committee, which supervises the universities' use of financial aid, states quite frankly in its latest report that "no serious measure of further improvement is likely to be secured, unless the universities have considerably increased funds placed at their disposal."

Whenever grants are made to aid research, freedom from restrictions is always the greatest aid to free-lance research. Freedom from restriction is therefore always more favourable to the production of results of far-reaching service. Competent research directed to a particular problem cannot fail to yield facts about the subject, but it may never solve that problem. Hence it comes about that a benefactor wishing to aid research in a particular subject must face a dilemma. If he decides to endow, say, cancer research he will aid the discovery of definite facts about cancer. These definite facts will be of service in the general study of that particular

subject. But in devising methods of treatment, results got in non-medical science by workers with no scientific knowledge of cancer may make all the difference between success and failure.

Both the directed and the free-lance research are necessary. As often as not the endowment of research in general, without restriction, has meant that the solution of a particular problem has been obtained. Whereas the restriction of the endowment to research upon that particular problem may be the very means of impeding its solution. To conduct restrictive research upon a particular *problem*, as distinct from a particular subject, is to try to find a particular thing in a particular place. If it is there, all is well. If it is not there, where are the rational grounds for surprise that the particular thing sought is not found? It may be sound common sense to make a direct attack upon a problem, but in the past, all scientific problems have not been solved by the 'obvious common-sense' (direct-attack) method.*

It must be admitted quite frankly that this inability of scientific research to guarantee a solution of a problem under conditions of restriction of any kind whatsoever, is extremely tantalizing to both the scientist and the non-scientist. To ask a benefactor to endow scientific research at all is asking him to endow the least spectacular of all charities. To ask for the endowment of free-lance research is to ask for almost superhuman beneficence.

So far in this discussion I have expressed views as an individual without making any claim whatsoever to express results representative of scientists in general. Nevertheless my statements upon the endowment of free-lance research appear to agree with those widely accepted. No more representative body of scientists can be found throughout the world than the Royal Society, whose official statements are expressed with exceptional restraint. [This society possesses or administers funds without which much university free-lance research would be either severely hampered or

* For examples in medicine see H. Dale, "The Relation of Physiology to Medicine in Research and Education," *British Medical Journal*, p. 1043, vol. ii, 1932.

prohibitively difficult.] In its Yearbook (p. iv, 1936) a memorandum as to the wishes of the Council in respect of benefactions to the Society is published and includes the following statements: "The President and Council have again and again had the experience that the usefulness of the Society for the advancement of Natural Knowledge has been greatly hampered by the lack of funds, of which they could freely make use according to their own judgment." Reference is made to "funds tied down by no special directions as to their applications, funds which the Society are free to use for general purposes," and the memorandum ends: "The President and Council accordingly desire to make it generally known that, while they will willingly receive gifts to be applied to special objects or for the benefit of particular sciences indicated by the donors, they consider that, in view of the varying necessities of Science, the most useful benefactions are those which are given to the Society in general terms for the advancement of Natural Knowledge."

If in my anxiety to be honest with the general reader I have at times over-emphasized the limitations of science, let me here emphasize its production of permanent things. The generous benefactor who builds and equips a laboratory gives research its essential home. But the generous benefactors who endow free-lance research 'for the advancement of Natural Knowledge' in that laboratory, are helping to produce the most permanent of man's products. For will not the endowments result in the research workers' manipulating apparatus in their hands and theories in their heads until facts are produced? In time the bricks and mortar will crumble, most of the theories will have gone out of fashion, but, remaining as man's firmest anchor to stability will be the facts.

THE FUTURE

To end a scientific study of the scientist in action with a discussion of the expense of experimental research may seem sordid. But the research worker has throughout been treated as reacting to his environment, and the influence of finance

on laboratory facilities, and therefore on the scientist's environment, is profound. The benefactors who founded the Royal Institution were as much a part of the 'circuit' which was completed by Faraday as was Faraday himself. Who can say which part of Faraday's environment might have been omitted without altering any of Faraday's actions?

The problems here discussed affect the whole of civilization. There are research workers who, whilst deprecating the connection of science with politics, are nevertheless deeply sensible of their social responsibilities. Such workers are led to suppose that they may best help by applying the results of science to practical affairs. Practical men of affairs often say that academic science is all very well in its way, but that the real public benefactor is he who shows how to make two blades of grass grow where only one grew before. Those who set to work to make themselves 'useful' now find that food crops and production have been increased only to be burnt or destroyed whilst simultaneously a body of medical scientists, the '5s. 10½d.' Committee of the British Medical Association, spends a good deal of effort in determining the minimum rather than the optimum food requirements of an unemployed man.* Both the Industrial and the Research Association laboratories are better adapted to apply science than are the universities. Because of these and many other similar facts it is unreasonable to suppose that at present the science departments of universities can best help the community by increasing the bulk of industrial or applied science research.

At present civilization uses scientific facts but not scientific methods in practical affairs. The only untried method which has been proposed for enabling man to enjoy more of the blessings of applied science with less of its curses is that both the results and the methods of science should be directed to social uses. If the attempt is to be made to apply scientific action to social problems, individuals able to make such application must be found. In any given circumstances an individual's actions depend partly upon his biological constitution and partly upon his past experiences. Of these

* See *The Frustration of Science*, D. Hall and others 1935.

experiences education can play a large part in determining future action upon, and reaction to, the external environment.

In facing this problem of nature and nurture, attention will first be concentrated upon education and in particular upon the most elaborate scientific education in current use. It is comparatively simple to teach the majority of science students to manipulate scientific apparatus. But during their university training and after they have left the university, can they upon occasion manipulate their experiences scientifically? Some disconcerting facts must be faced. A man may be a science scholar without being a scientist. A university degree in science is, at the time of award, a guarantee of knowledge of facts and principles in one or more sciences. It is no guarantee of a scientific outlook either in a particular science or in general. In some exceptional cases there are men of sufficiently advanced scientific knowledge to be members of university or industrial science staffs who yet have never been observed to apply scientific method to even the subject matter of their own science. Before such widely acknowledged facts are used to 'prove' that no form of science training could give or favour the development of a scientific outlook, more facts must be considered.

Neither education nor a scientific outlook can be observed by the coincidence method. They cannot be added to an individual as pills to be swallowed or as serums to be injected with a hypodermic needle. The process of university education consists of putting the biological unit into a university and making elaborate changes in its environment over a number of years. The subsequent actions of the individual depend partly upon its biological constitution and partly upon its past and present environment.

The university environment of a science student therefore demands serious study. It influences not only the scientists who go into industry, but also those who teach science in the schools. The majority of science students cannot, when they have taken their degree, undertake a post-graduate course in research. If they are to be told anything of the

scientist's methods, the opportunity arises only in their undergraduate days. For many other students the opportunity arises only through the science teaching in their schools.

In so far as I have been able to find out by observation, inquiry, study of syllabuses, examination papers, and recommended textbooks, the current practice in universities is:

(i) Science, but no scientific method, is taught to science students, and

(ii) Scientific method, but no science, is taught to a small proportion of Arts students. This teaching is done by philosophers who, in general, have had little or no technical experience in either a biological or a non-biological science, and the teaching is separated from that of the student's main subject of study.

As this is not the only conceivable way in which science may be taught, it is relevant to examine other possible methods before ascribing wholly to biological constitutions the failure of present methods to foster scientific action.

Scientific studies have been made of the influence of training and practice upon promoting or increasing skill and upon use of methods or attitudes. Studies have also been made upon the *transference* of skill, method, or attitude learnt in connection with one subject, from that subject to another. A summary of current knowledge was made by the British Association Committee on Formal Training in 1930. In their report occurs the following passage: "The influence of conscious recognition has been made amply clear by recent experimental work. Here lies a principle which is of special interest to the teacher. A common element is more likely to be usable if the learner becomes clearly conscious of its nature and of its general applicability: active or deliberate transfer is far more effective and frequent than passive, automatic, or unintentional transfer. This

seems especially true where the common element is an element of method rather than of material."*

From such considerations it seems that our present methods of teaching science are ill adapted to favour the learning or practice of scientific method even in a particular science. Their failure is consistent with current knowledge in psychology and education.

It would be unjust to give an impression that our present methods of teaching science are more open to criticism than are any other educational methods. Science teaching is so new that there is some difficulty both in knowing what to ask of it and in supplying the demands. Tizard records that when he was at school "to take an interest in science was held to be a sign that you were not quite a gentleman." When scientists, in striving to develop methods of teaching science, were considering whether experimental illustrations would help, they were apt to meet with such objections as: "It may be said that the fact makes a stronger impression on the boy through the medium of his sight; that he believes it the more confidently. I say this ought not to be the case. If he does not believe the statements of his tutor—probably a clergyman of mature knowledge, recognized ability, and blameless character—his suspicion is irrational and manifests a want of power of appreciating evidence, a want fatal to his success in that branch of science which he is supposed to be cultivating."†

THE NEXT STEP

Present methods of teaching science succeed in developing knowledge of facts and ability to manipulate technical apparatus. It is only in recent times that some men have wanted new ways of patterning human experiences in practical affairs, and have suggested that science education may be a means of fostering scientific action in such affairs.

* See pp. 279–286 of the Annual Report, 1930, or B.A. Reprints, n.s. No. 25, 6d. A short bibliography of books and papers is given. The B.A. Report on Science Teaching in Secondary Schools, 1917, may also be consulted.

† Todhunter, *The Conflict of Studies*. 1873.

To this end the same facts which were used in criticizing science teaching can be used as the basis of the new methods of teaching science.

It would appear that to foster scientific action in a particular subject and to foster its transference from one subject to another:

(i) Scientific method must itself be taught. The teaching of the facts and principles alone of one or more of the particular sciences, although it may imply much of scientific method, is not enough to produce specially favourable conditions for the spontaneous learning of scientific method or for its subsequent transference from one subject to another.

(ii) Although special lectures may serve to arouse interest in the subject, scientific method must in general be taught in combination with a science or with some other subject in which its application is illustrated.

To discuss fully means by which new methods of teaching science might be developed for even one of the sciences would be a study in itself. Since I have emphasized the biological individuality of each scientist, such a discussion may be deferred until the omissions and logical inconsistencies in my analysis of the scientist's methods have been remedied by other scientists. In directing attention to an anomaly in our current methods of teaching science and in proposing a change I have tried to suggest one method by which science may be able still further to help mankind. If I have pleaded for more money to be given for the endowment of free-lance research directed to 'the advancement of Natural Knowledge,' for freedom *with* facilities, I have tried also to suggest how, by developing new ways of teaching science, scientists may repay this generosity.

Many of the problems of civilization are said to be due to human nature. "You can't change human nature!" But scientific studies have shown that certain features of this same human nature are so dependent upon the external environment that they may be transformed almost out of

all recognition by changes in the environment. What 'comes natural' to a man depends partly upon his previous experiences. Even under present conditions some men take to scientific action as ducks take to water. Scientific action has never been tried in major social problems. Traditional methods are still failing after centuries of trial. Is it not time to try the scientist's methods?

CONCLUSION

I PERSONAL BASIS

II SUMMARY

I. PERSONAL BASIS

BEFORE giving a summary of the trend and conclusions of the foregoing chapters, it may be well to add a few words of personal explanation, which are perhaps all the more called for because of the stress laid throughout on what I have called the 'biological factors' in research.

Explanation.—I regard all explanations as patternings of human experiences. So far man has found no patterning devices capable of arranging experiences of all types into one pattern or mosaic. In present-day science the units of the mosaic are restricted to human experiences of one particular type, namely coincidence observations, and emotional or philosophical patterns are not used. In serious discussion I am unable to attach any definite meaning to a claim that any particular explanation of a subject is *the* explanation or the correct, or true, or probably true, or real, or only scientific explanation of the subject. For me no explanation of a given collection of facts ever permanently closes the subject. If an explanation is logically consistent to-day, it remains so to-morrow; but the explanation which to-day pleases or helps in action may not do so in the future. Always the kind of explanation found satisfactory by any person depends upon the subject matter, the occasion, and the person himself. Kelvin's statement that "when I have made a mechanical model I understand a process" tells as much about Kelvin as about that particular way of explaining phenomena. One man's explanation may be another man's chaos.

In science there are "some minds which can go on contemplating with satisfaction pure quantities, presented to the eye by symbols, and to the mind in a form which none but mathematicians can conceive.

"There are others who feel more enjoyment in following geometrical forms, which they can draw on paper, or build up in the empty space before them.

"Others, again, are not content unless they can project

their whole physical energies into the scene which they conjure up. They learn at what a rate the planets rush through space, and they experience a delightful feeling of exhilaration. They calculate the forces with which the heavenly bodies pull at one another, and they feel their own muscles straining with the effort.

"To such men momentum, energy, mass, are not mere abstract expressions of the results of scientific inquiry. They are words of power, which stir their souls like the memories of childhood.

"For the sake of persons of these different types [scientific theory may] be presented in different forms, and should be regarded as equally scientific, whether it appears in the robust form and the vivid colouring of a physical illustration, or in the tenuity and paleness of a symbolical expression."*

I confess that I wrote this book as the result of an attempt to clarify my mind on the relationship between experiment and theory, and more especially on the relationship between experimental and mathematical physics. Although the outlook became so markedly biological, it is remarkable that for each of the differences between the treatment given in this and in other books a counterpart can be found in current mathematical physics. For instance, I reject the idea that science is based upon measurement—and mathematicians are developing non-metrical mathematics, using it successfully in crystallography and in atomic physics. I include the human observer in every product of science from fact to theory. Similarly, Einstein includes a mathematical idea of an observer in his results, whilst in much atomic (mathematical) theory, when the results are expressed in terms of the absolute theory of science, it has become necessary to speak of the 'observer' or the method of observation as 'disturbing' the phenomenon studied, and so preventing the 'true' state of affairs from being examined.

Traditionally, experiment and theory have always been linked by philosophy. I now see them as linked only by the human research worker. Logically, science is found to be

* Clerk Maxwell. Presidential address on "Mathematics and Physics," given at the Liverpool meeting of the British Association in 1870.

inseparable from scientists, but quite separate from philosophy. None could be more surprised than I was when this scientific analysis of the problem led me to a result so strongly biological. But I now regard the contents of section B of the Proceedings of the Royal Society as essentially relevant to the interpretations of the contents of section A.*

As compensation for the dependence of science upon scientists, it is found, for instance, that those essentially new results in atomic physics which have recently been claimed to lead to 'crises' and to revolutions in physical thought present no dilemmas to the patternist. He recognizes that the patterning devices which help an individual in his actions depend upon the individual, the subject, and the occasion. Hence there are no logical grounds for supposing that the patterning devices which help in action with sticks and stones will be adequate for all actions with X-ray tubes, spectrometers, Wilson Cloud Chambers, radio-active substances, and the like.

The entry of biological facts into this book seems to provide a striking illustration of the unity of methods in biological and non-biological sciences. Like most physicists, I received a scientific education completely devoid of any biological training. When, in studying the results of recent physics, it seemed necessary to take into account also the human observer, I still used the physicist's methods. It was not until I had decided what hypotheses might fit the facts about experimental research workers in action that I sought biological facts, outside physical research, to see if they also fitted the same hypotheses.

In trying to explain the results of, say, spectroscopy, I observe, coupled with the spectrometer, another object which holds a pen in its fingers and makes irregular marks upon a sheet of paper. Neither by training nor inclination was I fitted to regard this object as other than another piece of apparatus much more complex than the spectro-

* The Proceedings of the Royal Society are issued in two sections: (A) containing those papers which are of a mathematical or physical character, and (B) containing those of a biological character.

meter itself. In taking into account *all* the observable objects used in scientific research I still studied them as a physicist and not as a biologist. Although from a biological point of view this may have been putting the cart before the horse, the results are the same. The physicist's methods applied to the same material give the same results as the biologist's.

The 'pattern' idea was reached by the tortuous path of the electrical circuit, the use of group theory in crystal and atomic physics, and various experiences in the arts, instead of by the 'obvious' path of biological 'organization.' Until my manuscript was finished I knew nothing of Gestalt psychology, which seems to be based upon a similar idea. It is therefore amusing to notice what an excellent example I have unconsciously provided of the influence of previous environment upon the subsequent actions of an animal. It appears that Gestalt psychology has developed almost entirely on the Continent, where games play a minor part in the school environment. I was 'conditioned' in a school where, on Wednesdays and Saturdays, games were deified. Whereas I automatically discuss games as illustrations of the pattern hypothesis in explaining action, games are not even mentioned in Köhler's standard book, *Gestalt Psychology* (1930).

Truth.—Man is so made that he likes both some stability and some change in his experiences. In science the attempt to satisfy this human desire has commonly been made by using the idea of a constant stable kind of knowledge called truth or absolute truth, contact with which is made by using the idea of belief or faith, and by considering the various experiences of the laboratory, field, observatory, and study as steps by which the sum of this stable knowledge is approached. I have offered an alternative way of satisfying the desire by regarding as the stable part of science those experiences called coincidence observations (facts). Since facts are found in experience to be stable, this renders the idea of faith or belief unnecessary to give stability or contact with stability.

If that part of science which is regarded as stable be called

basic, then the traditional method is to take as basic that which at the time is consciously unattained, whilst I take as basic that which at the time is consciously attained. Whilst the traditional way is to regard the facts of science as something like the parts of a jig-saw puzzle, which can be fitted together in one and only one way, I regard them rather as the tiny pieces of a mosaic, which can be fitted together in many ways. A new theory in an old subject is, for me, a new mosaic pattern made with pieces taken from an older pattern. To speak of the one as being right, and the other as wrong, is then as inappropriate to the scientific as to the artistic mosaic.

II. SUMMARY

(The numbers in brackets are page references. The summary gives the main conclusions in *selected* topics only. Other subjects and references are included in the index.)

SCIENTIFIC research is a form of human action which gives two kinds of results, the human but impersonal observations called facts (18) and such arrangements (21) of facts as classifications, laws, and theories.

Three Barriers.—The first thing needed for a piece of research is a human being in action. Action in research seems to differ from common action in everyday life in three respects. Firstly, dealing with *new* (23) things is unavoidable in research. A study of human reactions to new things throughout the ages suggests a so-called Attack-Escape Principle (27), that on most occasions when most adults are first conscious of something new they either attack or try to escape from it. The principle seems to fit actions so diverse as the attack on the man who first carried an umbrella in London and the attack in 1900 on Planck's new quantum idea. The second respect in which research and everyday action differ is that the research worker makes no absolute statements about how things *should* or should not appear to behave (59). Thirdly, the widely used device of assessment of *value* (68) is not used in the practice of research. These three differences seem to act like barriers against the extensive use of research technique. In the present state of knowledge they may be called psychological barriers, as distinct from the physical barriers which so often prevent the application of the technique of experiment. The barriers seem to be in the human being rather than in those other parts of Nature to which the application of research technique may be desired.

Facts.—"Facts are the foundation of science, however they may be interpreted" (Faraday). The facts of science are

sense data. Both the contradictory accounts given by different unprejudiced eye-witnesses of the same event (78) and the phenomena of optical and other sense illusions (88) show that not all sense data are usable in science—where impersonal observations are wanted. An examination of human observation shows that agreement between different observers is readily reached if the different observers are set to judge coincidences (92). These elementary human judgments give the nearest to universal agreement that is ever reached and are called either *coincidence observations* or *facts.* The special sense in which these terms have been used throughout this book is explained in detail on pages 99–100. A coincidence observation is *not necessarily* a measurement, and an object is said to be observable (103) by the coincidence method *only when it can serve as one of the two things between which coincidence is judged.*

Coincidence observation is presented as a means of separating the sense data usable as basic in science from other sense data and from inference. In cases of doubt or controversy it serves as the final arbiter at the time it is applied. The statement that 'Science is based upon measurement' is rejected in favour of the form 'Science is based upon fact (or human judgment of coincidence).'

Selection of Facts (Chap. IX).—All research work deals with facts selected from observation of actual objects and events. The complete collection of observations, external and internal, seems to be unique in each individual. What is observed depends not only upon what is there to be observed but upon who is observing it. The observation of falling apples by Adam and Eve and by Newton differed. It is suggested that the selection of facts by an individual depends upon both his external and his internal environment (in the biological sense), and that what he observes on a particular occasion depends upon what he has previously observed (146). This hypothesis is suggested to fit such facts as that a given observer does not necessarily notice some fact about even a familiar object as soon as the object is familiar (134) to him, and that different observers with the same external environ-

ment do not notice the same facts about their external environment. In the history of science it is found that observers with excellent external facilities for observing some new fact have not in general noticed it until some one individual has first directed attention to it. Coincidence observations can be common to all when once they have been pointed out.

The selective processes of observation can give abstractions (154), but as these are always selected data it is never scientifically justifiable to say that an object is 'nothing but' the abstractions. Abstraction seems to help in dealing with actual objects, but, for example, triangular pieces of paper or wood cannot be manipulated by the hands in quite the same ways as the abstraction called a triangle can be manipulated by the mind.

Pattern.—In observing actual objects and events some of the selected data can be expressed as the addition of still more detailed data, but others cannot. The properties which cannot be seen as inherent in the properties of separate parts are called pattern properties, to express the idea of relationship between parts and a whole (120). The scientific devices of measurement (128, 284) and of cause-and-effect (291) relationship seem to be unsuited to express those properties of things which may be said to depend upon arrangement, or organization, or the totality of things. They are non-metrical and non-causal properties. Even the most detailed list of measurements does not express such an idea as symmetry (115), or any kind of shape or form, and it does not seem helpful to speak of any one part more than another as causing the symmetry or form. Illustrations are cited from crystallography (116), structural chemistry (119), atomic physics (118), literature (122), and music (126). Science has reached the stage where the study of parts and of wholes is needed in both biological (129) and non-biological (131) sciences.

Certain facts about ordinary observations (Chapter VIII), such as the direct perception of *change* without our knowing what detail has changed (139), suggest that the initial stage

in observation is the direct perception of a whole, which is later divided into other wholes as observation is continued.

In Chapter XI a wide range of human actions is analysed in terms of the conception that one of the characteristics of normal human actions is that they fit into or complete or make a pattern. In order to avoid the confusing influence of any subsequent rational meaning which the whole or pattern may have for the individual, a number of the actions cited are such as cannot readily be rationalized (195). The relevance of this discussion to the study of research technique is connected with the generally recognized fact that, although facts are the basis of science, "an accumulation of facts is no more a science than a heap of stones is a house" (Poincaré).

Arrangements of Facts.—Scientists are not satisfied with facts seen in isolation. The four most widely used devices for arranging facts in scientific research are classification, law, theory, and the cause-and-effect relationship. These devices relate to facts (coincidence observations), not to actual objects and events (224). Knowledge of the arrangements, however, influences human action in dealing with the actual objects. Policies of action by individuals with and without the knowledge of the arrangements differ even where the isolated facts are known to both.

In classification (178) grouping is made possible by seeing common similarities (149). In the natural law (173) the basis is the seeing of 'order' (167). In the sense in which research workers use the term, two observed facts or groups of facts are said to be related as cause-and-effect (291) only when observations have been made with the aid of the technique of experiment. The special form of experimenting used consists in making coincidence observations in pairs of experiments where one experiment of each pair is of the 'blank' or 'control' type (294).

In using the devices of classification, law, or causal relationship, the research worker studies actual objects and events, and his specific task is ended when he has made

such selections of data as show respectively similarity, 'order,' or presence and absence in pairs of experiments. In using the device of hypothesis or theory his task has only just begun when he has selected (225) his facts (coincidence observations). He seems to adopt a change of tactics and looks to his imagination (226) rather than to his observation to help him in his next step. The statements of what he imagines are called the *postulates* of the theory. The postulates of the theories in current favour appear to have the following characteristics in common:—They are free from logical inconsistency (226) and from ambiguity (233), and are as simple (238) and few as is possible to the theorizer. The statements are made in terms of things which cannot be observed by judgment of coincidence (242). The postulates may appear absurd (243) or contrary to common sense (246). When a theory is devised to deal with very diverse and varied facts, at least one factor is usually postulated as subject to quantitative variation (248). When pure reason is applied to the postulates, things observable by judgment of coincidence can be derived and are found to fit (253) the coincidence observations derived as sense data from the actual objects and events from which the theorizer started. Such theories are still more favoured if, when they are used as policies of action, they lead to the observation of facts additional to those considered when the theory was first formulated (254).

In Chapter XIV a number of special problems of theorizing are discussed. Chance, the Uniformity of Nature, and Probability are treated in Chapter III.

Experiment.—The technique of experiment is needed to give reliable, self-consistent (45–7) observations, and also to discover that particular type of arrangement of facts called *cause-and-effect* (291). For this latter purpose control experiments (294) are essential. Two widely used devices in experiment are the principle of going to extremes (288) and measurement (284). The description of the process of direct measurement of a length (284–6) is given in detail as an example of the patternist's device of *definition in terms of*

observable action instead of in terms of the philosophical idea of truth. (See also pp. 97-8.)

A number of special difficulties are inherent in biological research (299), and all experimental research is getting much more expensive than formerly (310). This favours specialization (308). Attention is directed to subtle but powerful factors which, in hampering free-lance research (318), cannot fail to influence civilization (Chapter XVI).

The majority of research workers write only technical scientific literature, but a minority, including some men of scientific genius, write also less technical accounts of scientific work. In this general writing (32) about the products of scientific research the writers agree about the basic facts of science, but they differ in their treatment of the various scientific arrangements of facts. These differences can be expressed in terms of two points of view, which I have called respectively the Absolute Truth or Inner Reality theory of science and the Description or Patterning theory of science.

Absolute Truth or Inner Reality Theory of Science (Chapter II).—This theory is the only one readily accessible to the non-technical reader and is adopted in all the widely read popular science books. Philosophical terms are freely used, but the minimum use is made of biological or psychological terms. The actual results of research workers are referred to in terms of an idea called Absolute Truth. This seems to be regarded as a kind of knowledge which is basic or constant or final, and which would be comprehensible to human beings. It is referred to in much the same way as is Godlike knowledge in ordinary language. The technique of scientific research is then regarded as a means of getting or of approaching this absolute knowledge, of getting to the bottom of things or of finding out what things are really and truly like. When one theory is replaced by another, the newer theory is said to be nearer to the truth than is the displaced theory, which is then regarded as wrong or as a mistake. A scientific law is held to be a rule obeyed by the parts of Nature concerned. Some classifications are called

natural, and some artificial. A logically sound theory which fits the facts is stated to 'prove' something about the 'existence' or 'reality' (263) of things mentioned in the postulates of the theory. Such a theory is referred to as if it were a creed, and such a phrase as '*believing* (271) in the reality of the things postulated' is freely used in general discussion of the theory. It would be regarded as illogical to 'believe' in more than one theory about the same facts at the same time. One of two alternative theories must be right, or at least it must be nearer to the truth than is the other. If the writer happens to be a mathematician and finds that certain differential equations or other mathematical forms fit the particular selected data in which he is interested, then it is claimed that mathematical forms express the inner reality of the phenomena and suggest that the Creator is a mathematician.

The Description or Patterning Theory of Science (Chapter II).—This theory is newer than the Absolute Truth theory, and is not at present used in popular science writing. It was adopted by Mach, Kirchoff, Karl Pearson, and Hobson, and in the form called the Patterning Theory is developed in parts of this book.

The Patterning Theory starts from the human research worker, and the idea of agreement between human observers (30) is used in place of the idea of Truth or Absolute Truth. From all the things of which a man may be conscious the particular sense data called *coincidence observations* (99–100) are used as the basic material of science and are called facts. No philosophical terms are used, and the language of observable human action is favoured rather than the language of inference (39–40). The various scientific arrangements of facts are regarded as patterning devices into which facts are fitted, and the part played by the human observer in each is explicitly stated. Thus classifications are stated to be based upon human perception of similarities (178). All classifications are human artifices (186). Published classifications stated to be 'natural' are found to show many points of similarity in properties in which the scientist is interested,

whilst 'artificial' classifications show few of such points. A scientific law (173) is a statement of 'order' seen by the scientist in the abstractions he makes from observing actual objects and events. A scientific theory is a conceptual system made by the theorizer as a kind of framework into which he fits facts (coincidence observations). It is a policy of action rather than a creed of thought. A logically sound theory which fits certain facts is not regarded as the only scientific way of looking at the selected facts (37).

Research workers take a dynamical view of the various patterning devices, and always ask whether they fit the facts or whether they 'work' when used as policies of action. The users of the Absolute Truth theory ask rather whether the devices are 'true' or 'natural.'

Applications.—Knowledge of the facts and arrangements of facts discovered by the application of research technique in the laboratory and field can profoundly affect human action. Civilization now depends upon the continued use of, and the continued extension of, this knowledge. Under present conditions the application of the results of scientific research in some circumstances adversely affects human happiness. It is suggested that the application of research technique itself to these problems may serve to remove this source of danger to human happiness. Apart from the three psychological barriers previously mentioned, there is only one device of research technique (the cause-and-effect type of experimenting) which is not widely applicable.

When any suggestion is made for dealing with certain problems of civilization it is customary to ask what should be done (63). Although there is nothing whatever in current research technique which, in any circumstances, enables even the most brilliant scientist to determine 'shouldness,' the present suggestion is not logically unsound (66–7). It is generally acknowledged that the problems of civilization will be solved or will disappear as the result of human action. The observable facts of human action do not fit the idea that action necessarily depends upon an initial determination of 'shouldness.'

If the scientists' methods are to be applied to problems of civilization, men able to make the applications must be found. The kinds of action which 'come natural' to a man depend partly on nature and partly on nurture. This raises the question: Can scientific action be fostered through education? It is generally acknowledged that the most widely used methods of teaching science foster neither a scientific outlook in a particular science, nor the transference of a scientific outlook from one subject to another (323). The failure of current science teaching in this respect is, however, consistent with current scientific studies of education (324), and the results of these studies can be used as the basis of changes in science teaching proposed (326) as likely to foster a scientific outlook.

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SCIENTISTS

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